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ABSTRACT

This report of a study on the influence of education on student achievement finds that while schooling does have some tangible effects, they are not enough to significantly counterbalance the effects of students' social backgrounds. The report is part of an extensive series of studies on compensatory education and its long-term effects. The study sample consisted of 15,579 first through sixth graders attending 242 public schools in the fall of 1976. Of these schools, 95 were studied over the next two years and their students were followed for up to three years. Data on students' home environments and outside activities were collected as well as information on the characteristics of each school. Basic academic skills were assessed by a set of standardized achievement tests. The data gathered were used in a series of structural-equation models of the schooling-achievement process. It was found that the effects of schooling are too modest to overcome the academic advantages of socioeconomically privileged students, and even in the early years when schooling is most effective, the linkage of school resources to background works to sustain if not increase the background-achievement relationship. It is suggested, however, that while compensatory education may not be able to alter this relationship substantially, it may make a difference to disadvantaged students by reducing the link between ability and resources and may increase their chances of completing school or enough school to participate successfully in society. Numerous data tables are used in the report. Supplementary tables, as well as detailed analyses of the study sample and of cohort patterns, are contained in the appendices. (CG)

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BACKGROUND, SCHOOLING, AND ACHIEVEMENT

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Technical Report 20
from the
Study of the Sustaining Effects
of Compensatory Education
on Basic Skills

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A GENERAL INTRODUCTION TO THE SUSTAINING EFFECTS STUDY
AND
AN OVERVIEW OF THE PRESENT REPORT

DESCRIPTION OF THE STUDY

In response to questions about education policies, SDC is studying compensatory education (CE); its nature, quantity, and environment; its sustained effects; and its generality, in a large study called: The Sustaining Effects Study. This thorough study will result in a series of reports from the following substudies:

The Longitudinal Study. In the Longitudinal Study, the growth of children in reading, math, functional literacy, and attitudes toward school were assessed in the fall and spring for three consecutive years. The amount and kind of instruction in reading and math was also determined for each student. In addition, teachers and principals reported on their practices of instruction and teaching. Thus, it was possible not only to assess student growth over a three-year period, but to relate this growth to ~~the~~ instruction.

The schools in the study were drawn from three different groups. The REPRESENTATIVE SAMPLE of schools is a sample carefully drawn to represent all of the nation's public schools that have some of the grades one-through-six. A second group of schools, the COMPARISON SAMPLE, is composed of schools that have large proportions of students from poor homes but do not receive special funds to offer CE services. The third group is the NOMINATED SAMPLE, composed of schools nominated because their educational programs had promise of being effective for low-achieving students. During the first year of the study, data were collected from 328 schools and about 118,000 students.

The Cost-Effectiveness Study. Information was obtained on the resources and services to which each student was exposed during reading and math instruction. Cost estimates were generated on the basis of this information. Because the effectiveness of the instructional programs is being determined in the Longitudinal Study, it will be possible to relate ~~the~~ effectiveness to the cost of each program.

The Participation Study. The purpose of the Participation Study was to determine the relationships among economic status, educational need, and instructional services received. The educational achievement of the students and the services they received were obtained in the Longitudinal Study, and the refined measures of economic status were obtained in the Participation Study. Visits were made to the homes of over 15,000 randomly selected students from the schools in the first-year REPRESENTATIVE SAMPLE. During these ~~the~~ visits, information was collected on the economic level of the home and on the parents' attitudes toward their children's school and learning experiences. Thus, the level of student achievement and services could be related to the economic level of a student's home.

The Summer Study. The Sustaining Effects Study also examined the effectiveness and cost-effectiveness of summer-school programs. Information about the summer school experiences of the students was combined with other data. The resource-cost model, developed for the regular-year, cost-effectiveness study, was adapted to the needs of the summer-school study.

Successful Practices in High-Poverty Schools. This study is intended to identify and describe instructional practices and contexts that appear to be effective in raising the reading and math achievements of educationally disadvantaged students. In-depth observational and interview data were collected from 55 schools ~~that are~~ participating in the study.

THE REPORT SERIES

The major findings of the reports already published are discussed briefly below, along with references to the specific study reports ~~from the study~~ that address them.

A Description of the Samples for the Sustaining Effects Study and the Nation's Elementary Schools. In order to understand the findings of this study, it is essential to become familiar with the characteristics of the samples used and their capabilities of providing generalizations to the population of the nation's schools. Technical Report 1 (Hoepfner, Zagorski, and Wellisch, 1977) describes in detail the samples and how they were formed. It also presents the results of a survey of 4,750 public schools with grades in the 1-6 range, ~~by~~ projecting the data to the nation. These projections accurately describe the nation's elementary schools in terms of characteristics of the school, the kinds of services the schools provide to students, and the characteristics of the students. The interrelationships among these characteristics are also addressed.

The different kinds of samples have been explained earlier in this review. Some results concerning the characteristics of the nation's public schools are summarized below:

- *Enrollment, Urbanism, and Achievement.* The total grade 1-6 enrollment in the 1975-76 school year was estimated at about 21 million students. There is a moderately strong relationship between school enrollment and degree of urbanism, with large cities having larger schools than rural areas, which tend to have small schools. The level of student achievement is related to the degree of urbanism in a complex way; in general, there are proportionally more schools in large cities than in rural areas that have more than half of their students achieving at least one year below grade level.

- *Compensatory-Education Funds, School Characteristics, and Achievement.* About two-thirds of the nation's elementary schools received Title I funds, and about one-fifth received no compensatory funds from any sources. There is little relationship between receipt of compensatory funds and the size of a school. However, small-city and rural schools tend to receive such funds more frequently than do large-city schools. As expected, schools with high concentrations of poor students tend to receive compensatory funds more often than do schools with low concentrations. Similarly, schools with higher percentages of low-achieving students are more likely to receive compensatory funds.

- *Achievement and Concentrations of Poor and Minority Students.* There is a strong association between percentage of low-achieving students and concentrations of poor and minority students.

- *School's Grade Span.* Generally, the grade span in the school has ~~small~~ weak relationships with the size of school, degree of urbanism, and concentrations of low-achieving, poor, and minority students.

- *Stability of Student Body.* Schools tend to have less stability in their student bodies as the size of the school increases, and there tends to be less stability in large cities. Similarly, stability decreases as concentrations of poor, minority, and low-achieving students increase.

- *Availability of Summer Schools*. Fifty-one percent of the nation's schools with grades 1-6 have summer-school programs available for their students. Larger schools provide summer-school programs more frequently than smaller schools do. There is practically no relation between the availability of summer school and a school's level of poverty, minority concentration, or level of achievement of the students.

A Description of Student Selection for Compensatory Services as it Relates to Economic Status and Academic Achievement. The Education Amendments of 1974 require several studies to inform Congress who does and who does not receive Title I services and how selection for such services is related to economic status of the family and the academic performance of the child (Section 417 of the General Education Provision Act). In addition, the federal program administrators want to know the differences between ~~the~~ services received by economically and educationally deprived children and those by non-deprived children, and the relationship~~s~~ between academic achievement and ~~the~~ children's home environment.

These questions were addressed in Technical Reports 2 (Breglio, Hinckley, and Beal, 1978), 3 (Hinckley, Beal, and Breglio, 1978), and 4 (Hinckley, Beal, Breglio, Haertel, and Wiley, 1979). A brief summary of answers to the questions is provided below:

- About 29 percent of poor students participate in Title I compared to about 11 percent of ~~the~~ non-poor students (Report 2). Looking at CE in general, about 40 percent of the poor students and about 21 percent of the non-poor students participate. From these findings, we can see that proportionally more poor students participate in the services than non-poor ones.

- Using the grade-equivalent metric (one year below expectation for the student's current grade) as the definition for educational disadvantage, about 31 percent of the low-achieving students participate in Title I, while only 10 percent of the regular-achieving students do (Report 2). For CE in general, the percentages are 46 for low-achievers and 19 for regular-achievers. Among the regular-achievers who participate in CE, many score below the national median on achievement tests.

- Participation rates for Title I and for CE in general are ~~the~~ highest for students who are both economically and educationally disadvantaged (Report 2). Forty-one percent of these students participate in Title I, and 54 percent participate in CE in general. Participation rates are next highest for students who are educationally but not economically needy (26 and 41 percent, respectively), and next highest for students economically but not educationally needy (20 and 28 percent, respectively). Only 7 percent of the students who are neither educationally nor economically needy participate in Title I (15 percent for CE in general). These participation rates were interpreted as indicating that the then-current allocation procedures were being complied with, and the intentions of the law were being met fairly.

- In comparison to non-poor students, poor students receive more hours of instruction per year with *special* teachers; more hours of instruction in medium- and small-sized groups; fewer hours of independent study; and more non-academic services such as guidance, counseling, health and nutrition (Report 3). The differences are even stronger when poor Title I students are compared to others. Therefore, we can conclude that the distribution of educational services is in line with the intent of the laws and regulations.

- Two aspects of the children's home environments bore significant and consistent relations to achievement: amount of reading done at home and the educational attainment of the head of household. Other variables, such as family size, TV-watching behavior, and type of living quarters were not consistently related to student achievement (Report 4). Although most parents (67 percent) know whether their children's schools have special programs for low-achieving students, few (40 percent) know of Title I and even fewer know of or participate in local governance of the Title I program. Poor parents, in general, are less involved in their children's educational programs, have lower expectations of their children's attainments, give lower ratings to the quality of their children's educations, but perceive Title I and other CE programs as being helpful.

Description of the Nature of CE Programs, Characteristics of Participating Students, Schools, and Educational Services. The Participation Study deals almost exclusively with what has been called 'selection for CE or Title I services', without examining too closely what such programs really are and how they differ from the programs regularly offered by the schools. Before we could draw any relationships between participation in a CE program and the educational progress of students, we had to be assured that there really was a program that was distinct, could be specified in some way, and had a reasonable chance of making an impact. As will be seen, not only did we analyze data on the basis of program participation, but we also considered the actual services received in order to address directly the possible differences between ~~the~~ intention and ~~the~~ actuality.

Based on the analyses of data obtained from about 81,500 students in the Representative Sample of schools, Technical Report 5 (Wang, Hoepfner, Zagorski, Hemenway, Brown, and Bear, 1978) provides the following important conclusions:

- Students participating in CE are lower achievers (mean score at the 32nd percentile) than non-participants (53rd percentile). Seventy percent of the participants were judged by their teachers as needing CE, while only 19 percent of those not participating were so judged. More minority students participate in CE, proportionately, than white students, but participation in CE has little relationship with student attitudes to school, early school experience, summer experiences, or the involvement of their parents in their educational programs.

- Minority, poor, and low-achieving students tend to receive more hours of instruction in smaller groups and by special teachers, and receive more non-academic services, but their attendance rates are generally lower too, so they do not take maximum advantage of the special services provided.

- The useful predictors of whether or not a student is selected to receive CE are his/her teacher's judgment of need and participation in CE in the previous year. When these variables are considered, achievement scores, non-English language spoken in the home, and economic status contribute little more to the prediction.

- About two-thirds of the students participating in CE in 1975-76 participated in the 1976-77 school year also.

- CE students in general and Title I students in particular receive more total hours of instruction per year than non-CE students. The CE students also receive more hours of instruction from special teachers. Among CE students, Title I students receive the greatest number of hours of instruction, more frequently with special teachers, and in smaller instructional groups. There are no significant and consistent differences between CE students and non-CE students with regard to their teacher's instructional subgrouping practices, use of lesson plans, extent of individualization of instruction, frequency of feedback, or assignment of homework.

- Students receive between 5 to 9 hours of reading instruction per week, decreasing steadily with higher grades, and between 5 and 6 hours of math instruction per week, fairly constant over all grades.

- CE services are delivered during regular instructional hours with different kinds of activities for the participants (so that, in effect, they 'miss' some regular instruction received by their non-participating peers).

- Title I schools have higher average per-participant CE expenditures in reading and math than do schools with other CE programs. The average Title I per-participant expenditure is about 35 percent of the average per-pupil regular (base) expenditure.

- Schools receiving CE generally have higher concentrations of poor students, ~~and~~ low-achieving students, and students with less educated parents. These schools ~~have~~ greater administrative experience and instructional control by their districts and have higher staff-to-student ratios.

- Schools that select higher percentages of regular-achieving students for CE services have larger percentages of minority and poor students, probably reflecting their tendency for saturation of CE programs.

- Most districts use counts of students receiving reduced-price lunches and counts of aid to families with dependent children to determine school eligibility for compensatory funds, while most schools select students on the basis of standardized achievement tests, frequently augmented by teacher judgments. Similar selection criteria are employed by non-public schools.

Cost-Effectiveness of Compensatory Education. In its deliberations for the reauthorization of Title I and in annual appropriation hearings, members of Congress also wanted information on the effectiveness of the Title I program relative to its cost. While it appears eminently sensible to ask the question of cost-effectiveness, it is difficult to provide ~~the~~ answers in a manner that will be interpreted correctly.

In the study of cost-effectiveness of CE, efforts were made to preclude enigmatic conclusions and, at the same time, to make cost estimates on a sounder basis than in the past. In Technical Report 6, Haggart, Klibanoff, Sumner, and Williams (1978) developed and presented a resource-cost model that translates educational resources for each student into estimates of average or standard dollar cost for his/her instructional program. The overall strategy for estimating cost is to provide an index that represents the labor-intensity of services without being confounded with regional price differentials, different accounting methods, etc.

Using the resource-costs, CE students in general, and Title I students in particular, were found to be offered substantially higher levels of educational resources, and hence more costly programs. Participation in CE differentiates the resource-costs for services offered much more than do poverty, achievement level, race, or any other characteristic~~s~~.

In Technical Report 7, Sumner, Klibanoff, and Haggart (1979) related resource-costs to achievement to arrive at an index of cost-effectiveness. Because of the low-achievement levels of the children participating in CE and their relatively slow rates of achievement growth, the increased cost associated with CE appeared to be misspent (in the same way that money for severely ill and terminal patients appears to be not as effectively spent as ~~it is~~ for mildly ill patients). It is important to point out, however, that the appearance may not tell the true story. Because we cannot obtain truly appropriate comparison groups, we do not know what would have happened to the achievement growth of the CE students if they had not participated. Based on the comparison groups we could form, however, CE programs did not appear to have an advantage over regular programs in terms of cost-effectiveness.

The Effectiveness of Summer-School Programs. The study ~~has~~ also examined the results of attendance at summer school, because members of Congress and program administrators wanted to know if such attendance helps prevent the presumed progressive academic-deficit of low-achieving students. If attendance at summer school has positive academic effects insofar as the attendees ~~will~~ do not 'fall back' to their achievement levels of previous years, then summer programs can be considered ~~as~~ a means of sustaining ~~the~~ school-year growth.

Technical Report 8 (Klibanoff and Haggart, 1980) show~~s~~ed that attendance at summer school has little or no effect on the academic growth of the students who attend, especially the low-achieving students. Because ~~the~~ findings are based on the study of summer schools as they presently exist (and the evidence is strong that they do not offer intensive academic experiences), the non-positive findings should not be interpreted as an indictment of summer schools, as such, but an evaluation of the way they are presently organized and funded. Nevertheless, when instructional services delivered in summer schools were investigated, none seemed particularly effective in improving students' achievement growth.

In the same report, the authors also addressed the hypothesis of 'summer drop-off', a hypothesis advanced to explain the presumed widening achievement gap between regular and CE students. Essentially, this hypothesis states that CE students lose much more of their previous year's learning during the summer recess than do regular students. Data collected in the study fail to support the summer drop-off hypothesis: CE students do not suffer an absolute 'drop-off' (although their achievement growth over the summer is less than that ~~for~~ of regular students, as in the school year). In any event, attendance at summer school does not have much ~~of an~~ effect.

Special Studies of Allocations, Achievement, and Attrition. A number of sub-studies ~~are~~ were presented in Report 13 (Hoepfner, Ed., 1981) that appl~~y~~ied selected data to specific policy issues or investigated in depth certain aspects of the complex data collected for the study. In response to the needs of Congress to have estimates of the numbers of schools and students that would participate in Title I under various changes in allocation procedures, national projections were made on such characteristics as poverty, region, and urbanism. Several sub-studies concentrated on how the poverty of a school or district is or can be gauged. The report also provide~~s~~d information on where and to whom Title I services were then (1976-7 being distributed.

Attending more closely to achievement as a basis for ~~the~~ distribution of Title I services, studies ~~are~~ were reported on the nature of "targeting" of services to students and how teachers reach judgments of their students' needs for Title I. Chapters also documented the methods for selecting and developing ~~the~~ STET measures of reading and math achievement, functional literacy, and attitudes to school that ~~are~~ were used throughout the study. The problems and advantages of out-of-level testing with low achievers ~~are~~ were also discussed, along with data from the study that illustrated the issues.

The samples for the longitudinal studies ~~are~~ were described in terms of the changes that occurred from the original first-year sample. Analyses of the attrition of individual students ~~are~~ were also presented and some conjectures provided about the expectable influences of the observed attrition on various analyses and findings ~~are provided~~.

- Continuing CE participants receive more, and more costly services than discontinued ~~ones~~ or regular students.

- Regular students show greater achievement growth than CE participants, who in turn, show greater growth than any of the discontinued (former) CE participants.

- Students no longer in CE show greater achievement growth during the first year out of CE than they did in the previous year, when they participated in CE.

CE Participation and the Achievement Gap. In Technical Report 12 (Zagorski, Conklin, Cooper, and Hoepfner, 1982), the achievement growth of CE participants and of non-participants ~~is~~ was followed for three years. Findings indicated that participation in Title I ~~leads~~ [illegible] to small but positive gains in achievement that are greater than we would have expected in the absence of Title I. Although the gains due to Title I ~~are~~ were not enough to lead us to expect elimination of the achievement gap within a reasonable number of years, they ~~are~~ were enough to slow down its widening and in some cases to reduce it.

To link achievement to Title I participation, students were studied year by year. The critical findings ~~are:~~ were

- Title I reading participants who improved and ~~are~~ were then discontinued from the program ~~do~~ did not fall back afterwards, but there ~~is~~ was a noticeable fall-back for math participants.

- New participants in Title I usually showed a recent history of achievement decline--and only a very modest reversal of that decline upon participation.

- "Chronic" three-year participants showed little improvement, and stayed at low achievement levels.

- The gains made by Title I participants cannot be accounted for by the amounts or types of educational services they received.

No striking evidence for the effectiveness of increased instructional services was found; nor were services found differentially effective for low and high achievers. Nonetheless, within the generally positive picture for CE, compensatory services were ~~are~~ more effective in improving achievement at the primary grades than at the later elementary grades.

Effects of Discontinuation of Compensatory Servies. According to the findings in Technical Report 11 (Kenoyer, Cooper, Saxton, and Hoepfner, 1981), each year about one-third of the CE participants ha~~ve~~d their CE services discontinued, mostly because of ~~due to~~ relatively high achievement. Although these students subsequently received reduced instructional services, their educational growth did ~~does~~ not revert to previous low levels or to the levels of current, comparable participants. No particular instructional services could be identified to ~~that~~ account for this continued growth. The tragedy ~~tradegy~~ of the disadvantaged young student who becomes deprived of the presumed benefits of CE is a disturbing individual vision not confirmed in our study of large groups.

About 60 percent of the students discontinued from CE programs were no longer qualified because of ~~due to~~ improved achievement~~;~~, 25 percent because their schools lost some form of CE funding~~;~~, and 15 percent because of promotions to grades in which there were no CE programs. Some specific comparisons among these groups of students showed:

- The achievement level of the second two groups (above) was substantially lower than that of the first group, and lower by far than that for regular students.

Technical Report 9 stet ~~is~~ a resource book. It identifie~~s~~d all the variables and composites ~~that have been~~ selected or devised for use in the Sustaining Effects Study. All measures and scales are described and rationalized. In addition, Report 9A serve~~s~~ stet as a companion volume ~~that~~ contain~~s~~ing copies of all the data-collection instruments in the study except for a few ~~that~~ ~~are~~ under copyright.

The Effectiveness of Compensatory Education and the Effects of Instructional Services on Achievement Growth. Technical Report 10 (Wang, Bear, Conklin, and Hoepfner, 1981) addresse~~s~~d the effects of compensatory services on student development. It also examine~~s~~d ~~the~~ instructional services and major dimensions of the educational process to describe the characteristics of programs that are effective in raising achievement. The analyses were based on the first-year data of the study. The central findings were that compensatory services have small but positive impacts on achievement--primarily in ~~at~~ the primary grades for reading, but in all the elementary grades for math. Looking specific~~ally~~ at educational services and processes, the major findings were ~~are~~:

- Regular instruction and tutor/independent work have small positive effects on achievement growth, while special instruction (small groups, special teachers, aides) do not.
- Achievement growth seems to benefit from use of more experienced teachers, more frequent feedback on academic progress, and stet more time teachers devote to preparation. It is hampered by classroom disturbances and by high concentrations of low-achievers in the school.

Successful Practices in High-Poverty Schools. The major objective of Report 16 (Lee, Carriere, MacQueen, Poynor, and Rogers, 1981) was to identify and describe the instructional practices that are effective in improving the reading and math skills of educationally disadvantaged students. Using intensive interview and observation techniques with the survey techniques employed in other aspects of the Sustaining Effects Study, the following factors were found associated with gains in achievement:

- Greater achievement occurs in schools where principal and teachers are more experienced and work together in harmonious and coordinated ways. Where teachers are more experienced, curriculum matches the content of achievement tests more closely, but this may be the result of teacher assignment policies.

- The more attentive students are during lessons, the better they perform on achievement tests. Attentiveness can be improved when teachers spend more time on instructional (vs. managerial) activities, where teachers are more satisfied and share educational views with their principal, and where teachers have responsibility for fewer students, so that less time is spent on independent (frequently off-task) activities.

- Teachers' common knowledge about and school-wide coordination of instruction ~~what is called~~ (coordinated instruction), is associated not only with achievement growth, but more directly with more active learning by students, better use of instructional staff, and more job satisfaction among teachers.

A structural model of the educational process showed how these and other factors are related to achievement.

In a *Description of Compensatory Services*, presented in Report 18 (Poynor, Surace, and Lee, 1981), compensatory programs were described in greater detail and were compared to regular programs in terms of classroom activities and practices. Even in the relatively small sample of high-poverty schools studied, compensatory programs were found to vary in many respects:

- There is variability in the regularity and duration of special services offered to students. While most schools offered daily services on a year-long basis, there were notable exceptions, both between and within schools.

- The nature of the special services differs widely in terms of staffing, use of materials, program emphases and techniques, relationship~~s~~ to the regular programs, and location of CE classes. Although about half of the programs reported using diagnostic-prescriptive techniques, the manner in which this was done varied considerably.

Some commonalities were also found:

- Although CE participants receive an overall higher amount of reading and math instruction, it is at the expense of parts of regular instruction.

- None of the ~~No~~ schools studied systematically monitored the progress of students once compensatory services were ended.

The various instructional settings were also studied. Pullout instruction was found to be associated with smaller instructional groups, more on-task behavior of students, more classroom harmony, a higher quality of cognitive monitoring by teachers, and greater organization of activities. Pullout provided more instructional time.

The attitudes toward compensatory programs were also investigated. Principals are generally well-satisfied with the effectiveness of their programs in terms of impacts on reading and math achievement. They perceived the impacts to spread to other subjects and to nonparticipants as well. Teacher's attitudes were more mixed. Negative attitudes stemmed mostly from the ways the programs were operated, and therefore appeared improvable by better understanding of program guidelines and improved administration. Little evidence was found for the stigmatization of participants in CE programs, either on the part of teachers or ~~of~~ fellow students.

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Studies Still to be Done. The reports yet to come from the study will address the general effects of educational practices on raising students' achievement levels, with special attention paid to the practices found in CE programs in general and in Title I programs in particular. Impact analyses will be based on three-year longitudinal data. The extensive achievement-data collected from overlapping cohorts of students in the three years will be used to describe ~~the~~ patterns of educational growth over the years for various groups of CE participants and nonparticipants. Analyses of the three-year longitudinal data will allow us to examine in greater detail the sustained effects of compensatory education programs.

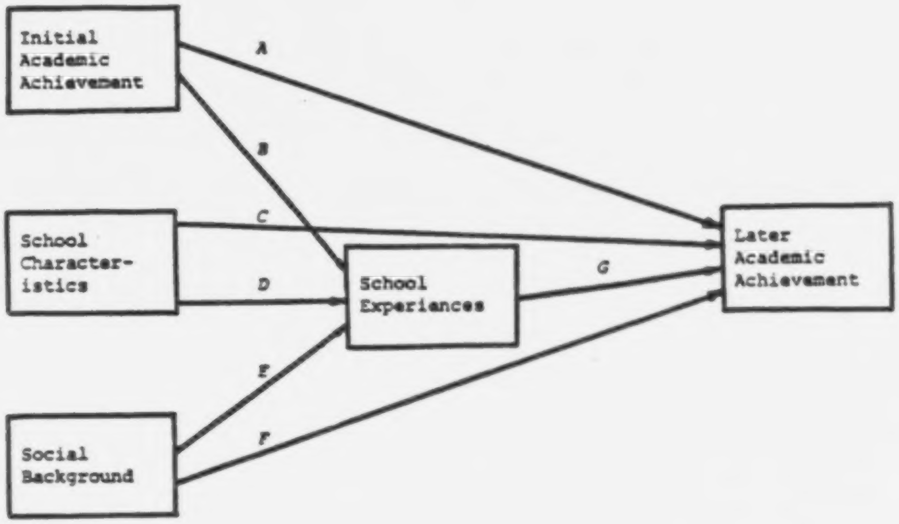
OVERVIEW

Fifteen years ago, *Equality of Educational Opportunity* (EEO) concluded that schools exert little influence on achievement that is independent of social background. Consequently, the report declared, "the inequalities imposed on children by their home, neighborhood, and peer environment are carried along to become the inequalities with which they confront adult life at the end of school."

These conclusions are disturbing and controversial, yet extensive reanalyses of the EEO data have failed to overturn them. The results of this study, drawing on extensive new data, qualify the earlier conclusions, but do not overturn them. Our analysis of the new data shows that schooling does have tangible effects on student achievement, but not enough to counterbalance significantly the effects of background.

Probably underlying some of the reluctance to accept the conclusions of the EEO report were two substantial limitations of the data. First, they were based on observations of students at a single point in time. Such data pose problems in establishing the direction of influence between schooling and achievement and in inferring the nature of students' experiences from comparisons among cohorts. Second, while background factors were measured at the individual level, school factors were measured at the school level. Thus, the influence of school factors was assessed without considering the large variation within schools in the quality and amount of resources that students receive.

With the Sustaining Effects Study (SES), we have the first national survey to provide longitudinal data on the school experiences and academic achievements of individual students. These data have been employed in the present study within a series of structural-equation models of the schooling-achievement process. The basic model is shown below.


Initial Academic Achievement
A
B
C
School Character- istics
D
School Experiences
G
Later Academic Achievement
E
F
Social Background

The model indicates that early achievement, schools, and background all influence later achievement directly (paths A, C, F) and also indirectly influence it through school experiences (compound paths BG, DG, EG).

Variables representing social background, school characteristics, and individual and classroom experiences of students were combined into composites and ~~were~~ then entered into the model as possible "causes" of achievement. In the process of developing the composites, we determined that the quantity of classroom instruction has a significant positive effect on achievement (part of path G) and that peer characteristics (parts of paths C and G) have a much weaker effect.

Most important, we found that the correlation between background and achievement is relatively constant (about .5) throughout the elementary years. On the other hand, our results clearly establish tangible effects of schooling on achievement. In explaining the coexistence of these two results, we point to the following findings:

- Academic skills are already closely related to social background at the time of entry into first grade.
- Educational resources are both distributed to students primarily on the basis of academic skills (path B) and the school attended (path D) together. To be sure, there is a significant amount of preferential treatment accorded to children of privileged backgrounds (path E), but there are still important opportunities for receiving school resources independent of social background.
- Despite the primary allocation of school resources on the basis of ability (path B), the relationship of initial ability to socioeconomic background results in a substantial association between background and schooling.
- While the effects of schooling are generally appreciable (paths G and C), they decline rapidly with increasing grade. The decline is particularly drastic for reading achievement, with no evidence of schooling effects in the later elementary grades.
- The most important determinant of a child's later academic success is his earlier abilities or achievements (path A).

Thus, schooling effects are too modest to overcome the academic advantages of socioeconomically privileged students. Even when such effects are at their maximum in the early years of schooling, the linkage of school resources to background, which is not only direct, but also indirect through the allocation of resources to those with greater academic skills, works to sustain if not increase the background-achievement relationship. Even if schools were successful in lengthening the period over which educational experiences had a substantial impact on academic skills, this would probably only augment the background-achievement relationship unless major changes were made in the way educational resources are currently distributed, to the point of according preferential treatment to those of disadvantaged backgrounds.

In conclusion, the results of this study suggest that even if we were to increase the amount of educational resources available to students to levels near if not beyond the limits of practicability, we would not increase their academic skills by much nor alter significantly the background-achievement relationship. If such results are disturbing, they are counterbalanced by three considerations. First, many other skills, personal qualities, and life events unrelated to intellectual and social origins and to prior educational and occupational achievements determine the economic success of individuals in our society. Second, substantive changes in the kinds of educational resources or in the way they are delivered to students may increase the effects of schooling independent of their origins. ~~And,~~ finally, while compensatory-education efforts may be unable to alter substantially the relationship between background and achievement, they may make a difference to those who are disadvantaged by background, by reducing the link between ability and resources. Such efforts, targeted at those ~~at~~ on the margin of stet

society, may increase the probability of completing school or enough school to participate more successfully in the economic, political, and social life of ~~the~~ our society.

CHAPTER 1. SCHOOLING AND EQUALITY OF OPPORTUNITY

Previous research on the effects of background and schooling on academic achievement concluded that schools are unable to affect the relationship between social origins and achievement that is established prior to the start of schooling. However, such research has been based on data with serious design limitations and has only partially delineated the factors accounting for the perpetuation of the background-achievement relationship.

The Sustaining Effects Study provides an unprecedented array of data on the achievements and school experiences of a national cross-section of elementary students for three years. These data are exploited in a series of structural-equation models that are elaborations of a basic model similar to that employed in recent studies of the adult socioeconomic achievement process. These models are used to explain the stability of the background-achievement relationship throughout the elementary period.

In the 1960s, a recurring assessment of American public education was that it failed, in one sense or another, to equalize achievement opportunities for children of different backgrounds (Clark, 1968; Coleman et al., 1966; Sexton, 1961). The most influential critical assessment, *Equality of Educational Opportunity* (EEO), concluded that

> . . . schools bring little influence to bear on a child's achievement that is independent of his background and general social context . . . . [T]his very lack of an independent effect means that the inequalities imposed on children by their home, neighborhood, and peer environment are carried along to become the inequalities with which they confront adult life at the end of school. . . . [E]quality of educational opportunity through the schools must imply a strong effect of schools that is independent of the child's immediate social environment, and that strong independent effect is not present in American schools (Coleman et al., 1966: 325).

This was not to say, the report noted, that schools have no influence on achievement, but rather that what influence they do exert is strongly linked to the student's social background. This conclusion has been reinforced and more emphatically stated by critical commentary and extensive reanalyses of the EEO study, which have argued that because the influences of background and schooling are so intertwined, the EEO report overemphasized the influence of social origins by attributing all of the shared influence of both factors to background (Averch et al., 1972; Bowles and Levin, 1968a, b; Mayeske et al., 1972; Mosteller and Moynihan, 1972; U.S. Office of Education, 1970).

While there is merit to this criticism, the EEO study was equally appropriately concerned with assessing the effects of schooling independent of social origins, as distinct from those effects that ~~only~~ enhance the background-achievement relationship because of STET ~~the~~ better schooling received by children of privileged backgrounds. Yet, it remains unclear whether the lack, if any, of a substantial independent influence of schools on achievement is due to the a modest effect of schooling on the development of academic skills, to the favorable distribution of educational resources to children of richer social backgrounds, to the strong, combined effect of background factors and initial abilities, which are highly correlated, or to some combination of these factors.

~~The~~ evidence to date explains only partially or with uncertainty the processes underlying ~~the~~ perpetuation of the background-achievement relationship. Reanalyses of the EEO data and subsequent analyses of other data indicate that school factors have some effect on the development of academic skills, but these studies have been unable to identify particular resources, programs, or environments (including school facilities, smaller classrooms, classroom hours, ability grouping, ~~or~~ and quality of teachers, curricula, or the student's peer environment) as being consistently effective (Averch et al., 1972; Jencks and Brown, 1975; Jencks et al., 1972; Karweit, 1976; Mayeske et al., 1972; Mosteller and Moynihan, 1972; U.S. Office of Education, 1970). More significant, the overall impression conveyed by these studies is that school influences are modest, although the quality of ~~the~~ data available has left these findings open to question.

In contrast, social background, however measured, has been consistently found to be strongly related to academic achievement (Averch et al., 1972: ch. 3). Yet this finding has been based largely, if not solely, on cross-sectional data, so that what appears as a high degree of influence of background on (later) achievement is in large part indirect, through the close association between background and earlier achievement. Of course, the inclusion of earlier achievement along with background in the prediction of later achievement, as permitted by longitudinal data, would still leave the relationship between the two predetermined factors unexplained, but at least would more accurately represent the effect of background on achievement over any given period and provide an indication of the degree to which the perpetuation of the background-achievement relationship depends on the effects of background net of initial ability.

Finally, it is clear that background is substantially related to the quality of school experiences, as well as to achievement. Placement in advanced tracks and curricula within schools, attendance at better financed schools with socioeconomically privileged schoolmates, and exposure to preprimary educational programs are all more likely to be experienced by children of privileged backgrounds (Coleman et al., 1966: ch. 2; Heyns, 1974; Hinckley et al., 1978: ch. 5; Jencks et al., 1972: ch. 2; Sexton, 1961; Wang et al., 1978: ch. 3). Although disadvantaged children are more likely to participate in compensatory-education programs (Hinckley et al., 1978; Wang et al., 1978), such programs appear unable to compensate fully for the educational experiences of socioeconomically privileged students (Wang et al., 1981). Consequently, to the extent that the schooling process promotes academic achievement, it appears to perpetuate initial, background-related differences without providing a significant means for advancing achievement that is independent of social origins.

It is the aim of the current study to clarify the effects of school factors on achievement, the extent to which the educational process perpetuates the background-achievement relationship established prior to entry into school, and the factors that account for the inability of schools to affect that relationship. Drawing on data that overcome two major limitations of earlier research, we show that school effects are appreciable during the very early years of the elementary period, but decline rapidly to extremely modest levels by the end of that period. Moreover, and more important, throughout the elementary period schools are unable to ~~affect~~ alter the background-achievement relationship because their effects, though appreciable, are either modest or, when otherwise, are linked both directly and indirectly to background through initial ability, itself the primary determinant of later achievement.

Limitations of Previous Research; Current Data

As politically disturbing and scientifically controversial as the EEO conclusions were ~~have been,~~ reanalyses and studies independent of EEO have failed to overturn them. Yet, there has been an understandable reluctance to accept the report's conclusions because of two major limitations, due in part to the relatively primitive state of educational theory (Bowles and Levin, 1968a, b; Cain and Watts, 1970; Michelson, 1970; Mosteller and Moynihan, 1972; Richer, 1975). As long as these limitations persist, it is possible that our failure to find strong effects of schooling may be due to weaknesses in the data and methods thus far employed.

Cross-Sectional Data. Most of the research to date has been largely or completely based on cross-sectional data, that is, data based on observations of individuals at a single point in time. (Notable exceptions are the various studies using the Project Talent data on achievement in high school and beyond

[Shaycoft, 1967; Jencks et al., 1972; Jencks and Brown, 1975]. Other longitudinal studies have examined high school stet effects on educational attainment rather than academic achievement [Alexander and Eckland, 1975; Hauser et al., 1976].) There ~~have been~~ are several serious problems posed by such data. First, we have been unable to determine the direction of causal influence between schooling and achievement. Thus, when we find that achievement is related to better schools, it is unclear whether higher achievement resulted from selection into better schools or whether selection into such schools resulted from achievement (via its association with socioeconomic status).

Second, trends in student performance and its relation to background and schooling as reported by the EEO study, are based on comparisons among cohorts at different stages of their educational careers, as if the experiences of these cohorts represented those of a single cohort advancing through school. It remains unclear whether the experience of any single cohort is at all similar to that constructed across the cohorts observed at any given moment. In particular, it is possible that differences among cohorts in the relation between social origins and initial academic abilities account for the cross-sectional pattern of the background-achievement relationship. Thus, the pattern may incorrectly indicate the relative longitudinal influences of background and school factors on achievement.

Finally, cross-sectional data on levels of exposure to school resources concurrent with achievement levels, may provide a poor indication of the total resources to which students ~~had been~~ were exposed up to the point of ~~the~~ achievement assessment. The resulting error in measurement of school experiences could increase the importance of background at the expense of school factors (Bowles and Levin, 1968a; Hanushek and Kain, 1972).

Measures of School-Resource Exposure. Equally detrimental to our past efforts in finding reliable schooling effects has been a deficiency in our measures of educational resources allocated to and used by individual students (Bowles and Levin, 1968a; Hanushek and Kain, 1972; Hauser et al., 1976; Richer, 1972; Smith, 1972). Because of difficulties in measuring resource use at the student level, almost all previous studies--cross-sectional and longitudinal--have employed school-level aggregates of resources, expenditures, or characteristics as measures of students' experiences, thereby ignoring the much greater differences within rather than among schools (Heyns, 1974: in particular, p. 1439, Table 1). By ignoring differences in resource exposure within schools, we have been forced to assess the relative influence of school and background factors among schools, with no assurance that this accurately represents the relative influence of these factors at the level stet of students (Robinson, 1950).

Until experimental data are available, our results will probably continue to be plagued by the confounding of background and school factors, making the assessment of *potential* schooling effects problematic. Nonetheless, we now have a fairly rich ~~student~~ body of longitudinal data on ~~the~~ school experiences and academic achievements ~~of students~~ stet that has enabled us to reexamine the schooling process as it is *currently* constituted. The data are from the Sustaining Effects Study (SES).

The three-year, multi-occasion design of the SES and its inclusion of multiple indicators of family background, individual educational experiences, and achievement has provided a data base that substantially overcomes the limitations of earlier data. The existence of longitudinal data on several cohorts of students for three years allows us to determine the temporal order of achievement levels, school experiences, and background characteristics and permits us to rely less on synthetic cohort constructions to view the academic careers of students. Second, the availability of data on the school experiences of individual students enables us to assess the relative influences of schools, background, and initial abilities at the level ~~of~~ students and to take into account the variation in students' experiences within schools.

A Model of the Schooling-Achievement Process

The SES data permitted a modeling of the schooling-achievement process along lines well established by Blau and Duncan's (1967) studies of the socioeconomic and academic achievement processes (Duncan, 1968, 1969; Duncan et al., 1968; Hauser, 1969, 1971; Heyns, 1974; Hauser et al., 1976; Jencks et al., 1972). The essence of their model, which is mathematically formalized in a structural-equation system or 'path' model, is simple: status achievement is viewed as a "temporal process in which later statuses depend, in part, on earlier statuses, intervening achievements, and other contingent factors" (Blau and Duncan, 1967: 202). In other words, the socioeconomic careers of individuals are viewed as a sequence or chain of achievement and achievement-producing events linked by direct and indirect influences on one another over time.

In the context of schooling, we suppose that the achievement process begins with the social and academic backgrounds with which students enter school. These 'exogenous' or 'predetermined' factors condition the child's school experiences--the curricula or tracks in which he is placed; the peers with whom he comes in contact; the teachers who serve as counselors, resource persons, evaluators, and role models; and the facilities, books, and equipment that are available to and used by him. In turn, his academic achievements depend on his school experiences, so that background and prior achievement influence later achievement through the schooling received. Finally, background and initial achievement directly affect later achievement (independently of the amount of schooling).

This simple conceptual model may be elaborated in several ways. First, if achievement and school experiences are measured ~~at~~ on several occasions, the schooling process may be viewed as composed of several temporally ordered stages of input, output, and feedback relations. Second, schooling and achievement can be decomposed into several subdimensions reflecting different areas or intermediate stages, as when peer and teacher influences are distinguished or achievement itself is conceived of as involving a motivational as well as cognitive component in a feedback relationship. And, third, the initial level of motivation can be taken into account as another exogenous factor, along with background and prior achievement.

Previous discussions of educational opportunity have been dominated by concern over the relative importance of background and schooling. The Blau-Duncan conceptualization of the achievement process in terms of a path or structural-equation model represents a significant departure from this tack. The path model shifts attention to the status-transmission process itself, as relevant to schooling as to the adult socioeconomic achievement process of which Blau and Duncan (1967: 202-3) write. Instead of evaluating the relative importance of different causal factors by partitioning the variance explained

> . . . attention is focused on how the causes combine to produce the end result. . . . [W]e can indicate, first, the gross effect of the measured background factors or origin statuses . . . on . . . achievement. We can then show how and to what extent this effect is transmitted via measured intervening variables and, finally, to what extent such intervening variables contribute to the outcome, independently of their role in transmission of prior statuses.

The research described in this report has been guided by the above conceptualization of the schooling process. The issue of the relative extent to which the process merely transmits the parents' status to the child's achievements--or provides an independent channel for development on the basis of initial achievement and motivation--is addressed by a series of structural-equation models that are elaborations of the elemental model described above. Given the models, the direct and indirect effects of background, schooling, and prior abilities on achievement are assessed, and issues relating to educational opportunities are addressed in terms of these effects. The models are based primarily on data for three years of four cohorts of elementary-school children in the SES. These cohorts were in grades 1 to 4 in the first year of the study.

The Data

The Population and Sample. The population surveyed under the SES consisted of the first to sixth grades in public schools in fall 1976. A sample of the six cohorts, one for each grade, was followed for as many as three school years, thus providing longitudinal data on student careers at overlapping intervals covering the entire elementary period.

The study sample is a national cross-section consisting of 15,579 elementary students attending 242 public schools in the fall of 1976. (This sample is known as the sample for the participation study and is described along with other, related samples in Appendix A below, Hoepfner et al. [1977], Breglio et al. [1978: appendices], and Hoepfner [1981].) The sample was drawn from a population of 20 million elementary students in 62,500 public schools. Of the 242 schools in the sample, 95 were purposively selected for follow-up in the next two years (1977-8 and 1978-9). The sampled students in these schools were followed as long as they proceeded through grades 1 to 6 in their schools during the three years of the study. They were not followed beyond the sixth grade or upon departure or during absence from the 95 schools. (Further details regarding sample, population, and sample weights are contained in Appendix A.)

Those students who were followed over at least one, two, and three years of the longitudinal survey are referred to as members of the one-, two-, and three-year panels, respectively. This study is based on data for the cohorts of students in the fall 1976 cross section sample and the three panels. The findings of substantive interest are based primarily on the data for the three-year panel of cohorts 1 to 4, that is, those in grades 1 to 4, respectively, in the fall of the first year, while some preliminary findings--prior to model fitting--draw on data for the cross section sample and the one- and two-year panels. (The one-year panel includes cohorts 5 and 6 and the two-year panel, cohort 5.)

Table 1-1 gives the original sample sizes by cohort (as of fall 1976) and the number of students remaining (after selection into the longitudinal survey and attrition) in the springs of 1977, 1978, and 1979.

The Data Base. This study drew on extensive data on students' backgrounds, schools and school experiences, and academic performance. The student's socioeconomic and family background, home environment, academic motivation, and outside activities relevant to academic growth were assessed by a parent interview during the spring of 1977. Parents' education, occupation, and

Table 1-1

Cross-section and Panel Sample Sizes by Cohort
(Grade in Fall 1976)

Sample	Cohort						All Cohorts
	1	2	3	4	5	6	
Cross-section (fall 1976)	2,757	2,490	2,483	2,380	2,442	3,027	15,579
1-year panel (spring 1977)	2,740	2,484	2,474	2,374	2,432	3,012[2]	15,516
2-year panel (spring 1978)	1,035	934	949	827	715[1]	0[2]	4,460
3-year panel (spring 1979)	857	793	759	557	0[1]	0	2,966

[1] Excludes 16 students held back one grade and still in the study in the third year (1978-79).

[2] Excludes 11 students held back one grade and still in the study in the second year (1977-78).

income; family size and structure; and race/ethnicity constituted the primary measures of socioeconomic background. The language spoken at home, the presence of reading materials there, and the parents' involvement in the child's school provided additional indicators of the home environment. Indicators of academic motivation were provided by a few outside activities reflecting student initiative, such as hours spent reading.

The student's school was characterized in terms of: the presence of certain facilities, expenditures, staff/student ratio, receipt of compensatory education funds, hours in the school year, use of half-day sessions, staff development activities, student body racial composition and academic achievement level, and the education and experience of the (reading and math) teachers and principal. The student's school experiences were recorded periodically with respect to the reading and math curricula in terms of: the number of hours of learning or instruction in various settings, participation in compensatory programs, and exposure to teachers of various characteristics and employing various practices.

Academic performance was assessed by means of a set of standardized achievement tests, the Comprehensive Tests of Basic Skills (CTBS). For most test levels, the reading subtests were in vocabulary and comprehension; the math subtests, in computation and concepts. These tests assess basic academic skills rather than knowledge specific to certain curricula. 'Vertical' scales were developed from the reading, math, and combined scores in order to assess academic growth over grade and test levels (Hemenway et al., 1978: ch. 1). These scales ~~are~~ were used in this study. They show high internal consistency (Hemenway et al., 1978: 42, 44) and moderately high test-retest reliability between consecutive semesters and years (Hemenway et al., 1978: 47; Appendix B below).

Details regarding the selection and construction of the instruments for academic performance and school experiences, their administration periods, and the reliability of the variables and scales constructed from them ~~are~~ were provided by Hemenway et al. (1978). The instruments themselves (except for those under copyright) ~~are~~ were reproduced along with the parent-interview questionnaire in a report of the SES Staff (1979).

CHAPTER 2. THE COMPONENTS OF BACKGROUND AND SCHOOLING

> To assess the relative influences of background and schooling on achievement and the manner in which schooling and achievement affect one another over time, composites representing background factors, school characteristics, and individual-and-classroom school experiences were constructed. In the course of developing these composites, the components of background and schooling that influence achievement were examined. The most important variable of the home environment that appears to affect achievement is the kind of educational support and encouragement the child receives. ~~At school,~~ there is no support in the current data for the proposition that the peer environment is the most important influence at school or even that it is a major and consistent influence. On the other hand, ~~the~~ sheer quantity of instruction has a significant, if modest, effect on achievement averaged over the elementary grades.

A central question of interest, prior to the issue of the relative influences of background and school factors, concerns the components of these factors that encourage learning. Two school factors that have received some recent attention and on which we can bring some data to bear are the character of the student body and the amount of classroom instruction to which a child is exposed. In particular, a major conclusion of the EEO report was that the peer environment was the most important school factor in accounting for school-to-school differences in achievement. Furthermore, Wiley (1976) has suggested that the quantity of instruction has a significant impact on academic performance. To date, there has been no consistent evidence of positive effects of any school input and what evidence has been offered has been subject to question.

We assessed the influences of various school and background factors in the course of developing composites for use in structural-equation models of the schooling process. These composites represent background (B), school characteristics (S), and classroom and individual educational experiences (X). (For reasons discussed below and in Chapter 3, 'schooling' ~~is~~ was measured by composites at two levels--the level of the school and the combined levels of the individual and classroom.) The composites ~~are~~ were used in the models to assess the relative influences of background and school factors and to determine the manner in which they act through time and are conditioned by intervening achievement statuses.

The primary motivation behind the development of the composites was to reduce the number of variables considered. This reduction not only simplifie~~s~~d representation and interpretation of the effect of each set of factors in the models, but also reflect~~s~~ed a reluctance to place much reliance on the effects of the individual components. This reluctance ~~is~~ was the result of two facts. First, the effects of the variables evidence little stability across studies and are frequently negative because of the substantial intercorrelations among the variables. Second, the variables themselves only partially represent the relevant factors in any given area and are often proxies for factors in other areas as well, thus making any strong interpretation of their effects problematic.

A problem with the use of composites, however, is that they tend to oversummarize the data in two respects. First, they leave unanswered questions about the particular underlying factors that are important. Second, their construction from components of varying units of measurement makes it difficult to interpret patterns of cohort similarities and differences at the composite level. Thus, in the course of describing the construction of each of the three composites, we will examine the underlying factors that appear to affect achievement. The variables included in the composites are used to describe and compare the backgrounds and educational careers of the cohorts in Appendix B.

The Variables Considered in the Composites

The variables considered for each of the composites reflect the cumulative evidence of previous research, but are limited by the data available. The evidence of earlier studies on the effects of family background are strongly suggestive of the kinds of advantages that accrue to children of higher socioeconomic backgrounds (Coleman et al., 1966; Blau and Duncan, 1967). The important background indicators reflect the degree to which the family carries out various educational support functions, such as socialization of academic motivation and encouragement of achievement, teaching and modeling of academically relevant skills, and provision of a stable socioemotional and financial environment. The family's resources for and commitment to these tasks are reflected in its socioeconomic status, its race/ethnicity, its structure (number of parents present and number of siblings), and its behavior (provision of educational materials, encouragement, assistance with homework, and aspirations for the child).

While the evidence regarding schooling components has been less certain, it is reasonable to suppose that support, models, and intellectual resources are provided and goals set not only at home but also in school, where teachers and peers are the chief socialization agents. This presumption is supported by, among other evidence, the influence of friendship choices at school on post-schooling aspirations (Campbell and Alexander, 1965) and the substantial dependence of socioeconomic achievement on educational attainment, which in fact mediates the influence of social origins (Blau and Duncan, 1965: ch. 5).

Ideally, we would like to have individual-level data on school experiences along all the significant dimensions. Because of practical difficulties in procuring such data, school and classroom characteristics must be employed along with individual-level data. For example, where information on students' use of school facilities or on the backgrounds of those peers who serve as referent individuals and resource persons is impractical to obtain, the facilities of the school or the average socioeconomic and academic statuses of schoolmates, or better, classmates, is used.

Within the study, there are class- and individual-level data on some school experiences. The student's attendance and hours of schooling (~~over~~ in all courses) in each year were recorded. In reading and math~~,~~ such data were gathered by classroom setting and size and by type of instructional staff. The students' reading and math teachers reported their professional backgrounds and teaching practices, such as use of ability (sub)grouping of classes and amount of homework assigned to students. Individual-level data were also gathered on participation in compensatory reading and math programs. Finally, students were linked to homeroom teachers, thereby facilitating the treatment of class differences within schools and the measurement of classmate as well as schoolmate characteristics.

Among large-scale surveys, these data are a significant improvement in the measurement of exposure to school factors, but they likely capture only a small part of the variation in the quality of school experiences among and within schools. In particular, we still know little about the kind and degree of attention a student receives in the classroom (despite teacher reports of some classroom-wide practices), about the nature of school programs in which he participates, or about his actual use of resources and services. For example, while our data on quantity of classroom exposure includes number of hours attended as well as offered, we do not know the number of hours of class *attended to* by the student.

In the absence of an adequate representation of school experiences, it is possible to misrepresent two important relationships in the schooling process. First, it is possible to overestimate the influence of background on achievement, since some aspects of the child's social origins may serve as proxies for unmeasured school experiences. Second, it is possible to misrepresent the overall direction of the relationship between background and school inputs. Some inputs (e.g., 'gifted' programs, skilled teachers, motivated peers) are more likely to be received by children of privileged backgrounds, while others (e.g., compensatory or remedial programs, special teachers for the academically handicapped) are more likely to be received by socioeconomically disadvantaged children. If the inputs measured are predominantly those more likely to be received by privileged children, the overall association between background and school-resource levels will be positive. On the other hand, the relationship will be negative if the inputs measured are on the whole more likely to be received by disadvantaged children.

We have no complete cure for these problems, but to try to take into account class and school characteracteristics that are not measured, aggregate teacher and peer characteristics will be used as surrogates. Of course, the surrogates may capture only part of the unmeasured variability in class and school quality and, more important, their effects cannot be attributed solely to teachers or to the school environment as against other school factors.

Construction of the Composites

Each composite was formed by regressing later achievement on earlier achievement, cohort (a categorical variable), and a set of components representing background or schooling factors. The inclusion of cohort in the regression meant that the effects of the remaining variables were within-cohort effects.

These effects were assumed to be invariant across cohorts, so that ~~the~~ selection of the components and ~~the~~ calculation of their weights were based on the average experience of the cohorts. A description of the code categories for some of the variables considered, whose scales are not apparent, is given in Table C-1 (Appendix C).

In the case of the school-experience (X) composite, data on the components were gathered each year, and thus the 'earlier' and 'later' achievement levels were the fall and spring of each school year for each cohort for which data were available. For the other two composites--background (B) and school characteristics (S), the 'earlier' and 'later' achievement levels were fall 1976 and spring 1979, respectively, for the three-year panel of cohorts. The background data were gathered only once (in the first year) and school characteristics were not only similar from year to year but also were conceived of as exogenous (and therefore fixed prior to the first exposure to classroom and individual educational experiences and to subsequent events in the student's educational career).

Although the 'later' achievement level used for the B and S composites could have been the spring 1977 measurement, the spring 1979 measurement was used to allow background and school characteristics to exert their influences over the entire span of the years observed. Because ~~Since~~ earlier achievement accounts for a larger portion of the true-score variance in later achievement as the intertest interval diminishes, there may be some concern that the different regression procedures for the X composite on the one hand and the B and S composites on the other, in some sense favor the latter. To be sure, more components are likely to be included in the latter composites, resulting in greater proportions of explained variance in later achievement net of initial achievement. However, in the first place, as shown below, in no case did the addition of a few components at the end of the stepwise procedures dramatically increase the explained variance in later achievement net of initial achievement. Second, it seems reasonable to consider the long-term impact of relatively stable attributes such as background and school characteristics, which are likely to exert continuing influences throughout a child's educational career, while considering only the short-term impact of classroom and individual educational experiences, which are much more variable over the years and whose impact~~s~~ stet ~~are~~ presumably largely immediate. Finally, as noted below, a more generous criterion of inclusion was employed for the X composite than for the B and S composites allowing ~~to give school experiences the~~ maximum opportunity for school experiences to account for achievement.

In forming the composites, each set of variables was considered independently; that is, the regression analyses for the background composite did not include school factors (for X and S) as regressors, and, similarly, the analyses for each of the X and S composites did not include the background variables or the variables of the other school factor. Because of the correlation among components for the three areas, this procedure no doubt affected the regression weights. For example, in the construction of the X composite some components probably functioned in part as proxies for background factors, and the regression coefficients for the school inputs alone probably represented the effects of both types of factors. Nevertheless, other factors were excluded in each case in order to give the components in each area the maximum opportunity to explain achievement gains.

A stepwise procedure (maximum R^2 improvement) was employed to determine which components significantly contributed to the explanation of achievement gains, or, more precisely, of later achievement net of initial achievement (within cohorts). Because of the large sample size, addition to the adjusted coefficient of determination (of at least .001)--rather than statistical significance at conventional levels--was generally ~~relied on~~ used to determine the cut-off point for entry of a component into a composite. Adjusted R^2, rather than R^2 alone, was used in order to discount the additional explanatory power of components by the degrees of freedom they consume, or conversely, the additional complexity they introduce into the composite.

However, for the X composite, statistical significance at conventional levels was employed since the more restrictive criterion would have meant that no components would have entered at all (see Table 2-6). ~~and since~~, given previous findings of limited schooling influences, we intentionally sought to give school experiences as much opportunity to account for achievement as possible, that is, we preferred to over- rather than underestimate the effects of schooling if any error were to be made. (A more generous cut-off in terms of adjusted R^2 is indicated for the X composite in view of the fact that the adjusted R^2 with initial achievement and cohort taken into account is substantially greater [.90] than for ~~in the case of~~ the B and S composites [.71]. However, even if the same selection criterion in terms of the multiple partial coefficient of determination STET ~~were~~ used for the X composite as was used for the other two, none of the school-experience variables would have entered their composite.)

In the case of the B and S composites, an additional stepwise procedure was employed with fall 1976 achievement as the criterion and cohort among the predictors. The 'significant' predictors from this procedure (excluding cohort) were added to those selected by the first procedure, and the regression weights were recomputed using as the criterion later achievement net of initial achievement and cohort. For the B composite, the purpose of adding the components from the second procedure was to construct a composite that would also be useful in addressing the question of the extent to which academic and social origins are related. For the S composite, which could also function as a proxy for background (neighborhood) factors, the second procedure was similarly motivated. Again, it is clear from the results reported below that the addition of these components did little to increase the explanatory power of the two composites at the expense of the X composite with respect to achievement gains.

Of course, there are several alternat~~ive~~e approaches to constructing the composites, among them, factor analysis, leading to the development of factor scoring weights; standardizing the components and applying unit weights (Wainer, 1976); constructing composites by embedding the components in the causal models and employing the method of sheaf coefficients (Heise, 1972); and, in the case of school inputs, employing weights representing relative costs of the components where possible (Haggart et al., 1978; Sumner et al., 1979). All of these alternatives were considered, but we were interested in employing weights that (1) were simple to obtain; (2) would be easily developed for the cohorts as a whole throughout their educational careers, so that the interpretation of the composites would be invariant across cohorts and grades; and (3) would represent in some sense the relative effectiveness of the components in producing achievement gains. In addition, in the case of school inputs, weights based on relative effectiveness were of interest in order to assess "'effective' inequality of opportunity" (Coleman, 1972: 148), that is, the extent to which resources measured in terms of their effectiveness--instead of their absolute amounts or costs--have been differentially allocated to children of differing backgrounds. In any event, it is strongly suspected that the thrust of the conclusions drawn would not be significantly different whatever the weighting scheme (Wainer, 1976; Sumner et al., 1979: 26-7; Wang et al., 1979: ch. 3; see also the discussion of the X composite below).

A possible problem with the weighting scheme is that the composite constructed is used in the causal models to predict achievement, thus introducing the possibility that a high dependence of later achievement on one factor, say, school experiences, as against other factors, may be built into the X composite. The concern here is that we hypothesize that school experiences cause achievement and use the degree of achievement to define the degree of schooling. There are two considerations that would seem to mitigate this problem. First, all composites were constructed in a more or less similar manner (except as noted above); thus, insofar as the relative importance of the various factors is of concern, each composite is given an opportunity to account for achievement in a similar manner. Second, in the case of classroom and individual educational experiences (and probably in the other cases as well), there is little difference in predicting achievement however the components are weighted (Sumner et al., 1979: 26-7; Wang et al., 1979: ch. 3).

The B (Background) Composite

The Components. The B composite consists of eight components selected from a set of 12 that were chosen to represent background:

(1) Father's education (FATHER),
(2) Mother's education (MOTHER),
(3) Occupation of the household head (OCC),
(4) Family income (INCOME),
(5) Race/ethnicity (of parent respondent--RACE--classified as "majority" if white or Asian but not of Hispanic origin and "minority" otherwise),
(6) Number of parents present in the home (2PARENTS)
(7) Number of books in the home for the child at his reading level (BOOKS), and
(8) Parents' attendance at school events (ATTEND).

These variables reflect both the socioeconomic status of the child's family and the kind of support he receives that motivates and facilitates his learning. (The other four variables considered will be described shortly.) Many of these indicators have also been found by the EEO report to represent well the backgrounds of students ~~well~~ (Coleman et al., 1966: ch. 3). The RACE variable was constructed to reflect the similar academic experiences of whites and Asians (Coleman et al., 1966: ch. 1).

Table 2-1 displays the pooled within-cohort correlations among the eight indicators and between them and academic achievement. Although there is some variation among the cohorts in the correlations, the pooled correlations reveal the basic patterns faithfully. First, it is evident that most of the indicators are substantially interrelated and related to academic achievement as of fall 1976 (CTBS0) and spring 1979 (CTBS3). Thus, it is not

Table 2-1

Pooled Within-Cohort Correlations of Background Variables and Achievement for Cohorts 1 to 4

Background and Achievement Variables	1	2	3	4	5	6	7	8	9	10
1. Father's education (FATHER)	-									
2. Mother's education (MOTHER)	.62	-								
3. Occupation of household head (OCC)	.51	.40	-							
4. Family income (INCOME)	.41	.41	.29	-						
5. Race/ethnicity (RACE)	.19	.26	.19	.37	-					
6. Presence of 2 parents (2PARENTS)	-.00	.10	.08	.43	.33	-				
7. Number of books in home (BOOKS)	.42	.40	.25	.34	.35	.21	-			
8. Parents' attendance at school events (ATTEND)	.25	.27	.15	.29	.26	.16	.26	-		
9. Total achievement, fall year 1 (CTBS0)	.38	.40	.25	.36	.36	.21	.44	.24	-	
10. Total achievement, spring year 3 (CTBS3)	.41	.42	.25	.33	.34	.20	.43	.23	.79	-

Note. — Each correlation is based on all cases in the three-year panel of cohorts 1 to 4 for which data are available for the pair of variables involved. The pooled within-cohort correlation is the pooled within-cohort covariance divided by the square root of the product of the appropriate pooled within-cohort variances. The pooled within-cohort variances and covariances are unweighted (i.e., simple) averages over the cohorts of the covariances and variances within each cohort.

surprising that once a handful of well-chosen indicators are selected, there is little to be gained in the prediction of achievement by obtaining further information (Table 2-3) at generally increasing cost.

Second, there is a great deal of stability in the relationships of the background variables to achievement over time within each cohort. This, together with the absence of any discernible trend in the correlations between background and fall 1976 achievement across all six cohorts (Table 2-2), indicates that the relationship between background and achievement is invariant as the child progresses through school. While earlier evidence (Coleman et al., 1966: ch. 3) for this proposition was based entirely on synthetic cohort data over the twelve years of schooling, the present data are for true and synthetic cohorts, albeit over a much shorter interval. Thus, intervening school experiences--however currently distributed with respect to family

Table 2-2

Correlations of Fall 1976 Achievement With Background by Cohort

Cohort	Family Background Characteristics							
	1	2	3	4	5	6	7	8
Cohort 1	.42	.41	.32	.39	.36	.21	.39	.31
Cohort 2	.40	.41	.29	.35	.35	.19	.48	.25
Cohort 3	.39	.40	.31	.38	.44	.22	.51	.25
Cohort 4	.40	.40	.29	.39	.38	.19	.48	.23
Cohort 5	.38	.40	.30	.36	.38	.22	.48	.26
Cohort 6	.39	.40	.30	.34	.36	.14	.46	.26

Note. — Family background variables are numbered as in Table 2-1. Each correlation is based on all cases in the cross-section sample for which data are available for the pair of variables involved.

background and effective in advancing academic skills--have little impact on the the relationship between background and achievement, a relationship that is well established before entry into elementary school.

By the same token, if schooling has little effect on the inequalities with which children begin their educational careers, it also does not reinforce these inequalities. It is important to recognize that, at the very least, the schooling process (as currently constituted) does not in any sense increase the predictability of greater academic achievement for children of higher socioeconomic backgrounds--at least not through the elementary years. In Chapter 3, we will examine the elements of the schooling process that tend to result in a perpetuation of the background-achievement relationship despite the effects of schooling on achievement and the existence of important opportunities for receiving school resources independent of social origins.

Finally, and perhaps most significantly, Table 2-1 reveals that the education of the child's parents and the availability of books in the home ~~for him~~ are among the strongest indicators of an academically supportive home environment. The single strongest predictor is BOOKS, a behavioral indicator presumably more subject to response error and (upward) bias than demographic indicators such as parents' education. We shall find in Chapter 3 (Figure 3-2 and Table C-2) that the correlation between the B composite and initial achievement for each cohort in the three-year panel is not much greater than the correlation between initial achievement and BOOKS alone for the cross-section sample (Table 2-2). (The exception for cohort 1 is not surprising, given the low reading levels of children at their entry into school.) This performance of the BOOKS indicator is particularly impressive when we consider that the composite consists of seven other indicators, all selected and weighted to maximize the prediction of initial achievement and achievement gains. In combination, the strength of BOOKS and parents' education over other socioeconomic background variables (such as INCOME) in predicting achievement (see also Table 2-3) suggests that the development of academic motivation (Katz, 1967), the valuing of learning, and the acquisition of academically relevant skills (such as learning habits and verbal skills) may be among the most important advantages that parents of higher socioeconomic status provide their children in the early years.

The stepwise regressions for defining the background composite entertained four variables in addition to those that finally entered the composite:

(1) Size of place of residence,
(2) Languages spoken in the home,
(3) Size of the sib group less than 18 years of age, and
(4) Presence of newspapers and magazines in the home.

It was determined by preliminary analyses that the sex of the parent absent in a one-parent home was not of statistically reliable consequence. (This is not surprising, given that there were few students in father-only homes, and thus there was not much variation in the sex of the parent absent.) But, in order not to treat the absence of either parent simply as missing data on father's or mother's education, a dummy variable indicating whether or not either parent was absent (2PARENTS) was included along with the parents' education variables (one of which was coded '0' in the absence of the relevant parent).

The Stepwise Procedure. Table 2-3 summarizes the results of the stepwise regressions of fall 1976 and spring 1979 achievement scores on the background components for all cases with complete sets of data. The table shows the variable entered at each step up to termination, the total adjusted R^2 for all variables entered up to that

Table 2-3

Stepwise Regressions for the Background Composite With Spring 1979 and Fall 1976 Achievement as the Criteria

Step	Component Entered	Adjusted R^2	Increase in Adjusted R^2
	Spring 1979 (N=1,831)		
1.	Fall 1976 achievement and cohort	.705	---
2.	Number of books in home (BOOKS)	.714	.009
3.	Mother's education (MOTHER)	.717	.004
4.	Race/ethnicity (RACE)	.719	.002
5.	Father's education (FATHER)	.720	.001
6.	Remaining 8 background variables	.721	<.001
	Fall 1976 (N=9,964)		
1.	Cohort	.740	---
2.	Number of books in home (BOOKS)	.788	.048
3.	Mother's education (MOTHER)	.799	.011
4.	Race/ethnicity (RACE)	.804	.005
5.	Father's education (FATHER)	.807	.003
6.	Parents' attendance at school events (ATTEND)	.808	.001
7.	Occupation of household head (OCC)	.809	.001
8.	Family income (INCOME; replacing FATHER)	.810	.000
9.	Remaining 6 background variables	.812	.002

Note. — The first stepwise procedure is based on the unweighted cases in the three-year panel of cohorts 1 to 4 for which data are available on the criterion and all the predictors considered. The second procedure is based on the unweighted cases in cohorts 1 to 4 for which data are available on the criterion and all the predictors considered.

point, and the increment in adjusted R^2 from the prior step. At the points where termination occurs, six components are included in the fall 1976 analysis and four in the spring 1979 analysis for a total of seven different variables. Adding the variable reflecting number of parents present in the home--a variable that is integrally related to parents' education as defined above, the B composite was built from eight components.

It is of interest that the variable entered in each of the first four steps is identical in each procedure. Apparently, as noted above, the number of books in the home for the child and his parents' education are among the most important indicators of the kind of home environment associated with academic achievement. This is true even for spring 1979 achievement after achievement and grade of the student in fall 1976, which account for most of the explained variance, are taken into account.

It is also of interest that a handful of background indicators suffices to account for most of the variance in achievement that can be explained by background variables (at least those considered). Even if one uses the multiple-partial coefficient of determination rather than R^2, with cohort taken into account, for CTBS0 the coefficient increases from .268 to .277--a gain of only .009--when the remaining six background variables are added after termination of the stepwise procedure. Similarly, for CTBS3, with cohort and CTBS0 taken into account, the increase is from .054 to .060--a gain of .006--when the remaining eight background variables are added.

The S (School Characteristics) Composite

The Components. The S composite consists of five components selected from a set of 20 chosen to represent school characteristics:

(1) The racial composition (percent white or Asian, but not of Hispanic origin) of the student's grade at the school (SCH-RACE),

(2) The average academic achievement of the grade in the school in the fall of 1976 (SCH-CTBS),

(3) The educational attainment of the school's principal (PRINCIPAL),

(4) The level of compensation given teachers for inservice training at the student's school (TRAINING), and

(5) The presence of a central library at the school (LIBRARY).

The remaining 15 school characteristics are we~~re~~:

(1) Presence of a reading resource center at the school,

(2) Presence of a math resource center at the school,

(3) Per-pupil expenditures,

(4) Size of regular instructional staff and administrative staff relative to enrollment,

(5) Size of special instructional and counseling staff relative to enrollment,

(6) Receipt of compensatory-education funds by the school.

(7) Number of hours of instruction in the school day, by grade,

(8) Use of half-day sessions by the school, by grade,

(9) Frequency of staff development activities,

(10) The school's reading and math teacher's average ecucation,

(11) Their average teaching experience,

(12) Their average extent of inservice training in their areas of specialization,

(13) Their average number of recent college courses in their areas of specialization,

(14) The principal's administrative experience, and

(15) His recent extent of 'staff development' training.

The Stepwise Procedure. Table 2-4 presents the results of the stepwise regressions for school characteristics and is parallel in form to Table 2-3. Although, as indicated in the table, only four variables should have entered the S composite, a fifth variable--LIBRARY--was incorrectly entered from earlier, preliminary results. This error was not detected until all analyses had been completed. However, the inclusion of LIBRARY in the composite in all likelihood had no effect on the conclusions drawn in Chapters 3 and 4. The correctly constructed composite correlated .89 with the S composite for all cohorts but the fourth, for which the correlation was .85. In addition, the improvement in the explanation of achievement resulting from inclusion of the fifth component was virtually nil, as indicated in Table 2-4. Finally, and most important,ly the generally small influence of the S composite on achievement that is manifested in the models of Chapters 3 and 4 would probably have been exhibited all the more had the additional component been excluded.

Table 2-4

Stepwise Regressions for the School Characteristics Composite With Spring 1979 and Fall 1976 Achievement as the Criteria

Step	Component Entered	Adjusted R^2	Increase in Adjusted R^2
	Spring 1979 (N=2,766)		
1.	Fall 1976 achievement and cohort	.709	---
2.	Racial composition of school by grade (SCH-RACE)	.714	.006
3.	Principal's education (PRINCIPAL)	.716	.001
4.	Teacher inservice training (TRAINING)	.717	.001
5.	Remaining 17 school variables	.720	.003
6.	School (92 levels, replacing the 20 school characteristics variables)	.726	.005
	Fall 1976 (N=10,032)		
1.	Cohort	.646	---
2.	Average fall 1976 achievement of school by grade (SCH-CTBS)	.739	.094
3.	Remaining 19 school variables	.739	-.000
4.	School (223 levels, replacing the 20 school characteristics variables)	.724	-.015

Note. — The first stepwise procedure is based on the unweighted cases in the three-year panel of cohorts 1 to 4 for which data are available on the criterion and all the predictors considered. The second procedure is based on the unweighted cases in cohorts 1 to 4 for which data are available on the criterion and all the predictors considered.

As in the case of background factors, a few of the 20 school factors are sufficient to account for the bulk of the explanatory power of all. Moreover, consideration of school as a qualitative factor did not yield a sufficiently large improvement in the explanation of later achievement, given initial achievement several years earlier, to warrant its use in lieu of the measures of school characteristics available. As for the explanation of initial achievement itself, the substitution of school for SCH-CTBS resulted in a decrement in adjusted R^2. (This is mathematically possible for two reasons. First, adjusted R^2, which takes into account the degrees of freedom for regression and the remaining degrees of freedom for error, can decline as variables are added to a model if those variables add little to R^2. Second, in the case at hand, even R^2 was less for cohort and school than for SCH-CTBS alone. Normally, this descrepancy would not occur, but here SCH-CTBS represents average initial achievement [CTBS0] by cohort [nested] within school for all students in the sampled schools, while the ANOVA model was fitted only to those students in the study sample [Table 1-1] and excluded cohort-by-school interactions.)

The results of the stepwise procedures for the B and S composites bear on two conclusions of the EEO report regarding the relative influence of background and school factors, and the kinds of influences that schools exert. If one compares the predictive power of background and school-level schooling factors with respect to initial achievement (fall 1976) or later achievement (spring 1979) net of initial achievement (Tables 2-3 and 2-4), it is clear that background characteristics are much more potent in two respects. First, despite the substantial correlations among the background variables, the improvement in prediction obtained from considering additional background variables at any given point in the stepwise regression is greater than the improvement with respect to school characteristics under the same conditions. Second, using the same criterion as a cut-off point, more background than school variables provide information about initial achievement and achievement gains. This superiority can in part be explained by the fact that the background variables are at the student level while the school variables are at the school-within-grade level. Nevertheless, even when two background variables--race and achievement--are aggregated to the school-within-grade level, they remain the most highly correlated with individual achievement, as shown in Table 2-5. Indeed, the magnitudes of these correlations are on the same order as that of the background variables, which are not aggregated, with achievement (Tables 2-1 and 2-2). Clearly, then, it is not simply the differences between levels of measurement that explain the differences ntal between background and school factors in accounting for initial achievement and achievement gains. Either school factors are less potent or we have measured the wrong ones and/or measured poorly the ones we have. The latter alternatives become increasingly less plausible as the number of studies considering diverse sets of school factors in appreciable numbers increases. It is likely that schooling influences, if stronger

Table 2-5

Pooled Within-Cohort Correlations of School Characteristics, Achievement, and Background for Cohorts 1 to 4

	1	2	3	4	5
School Characteristics					
1. Average achievement in school and grade, fall year 1 (SCH-CTBS)	--				
2. Proportion white or Asian in school and grade, year 1 (SCH-RACE)	.65	--			
3. Teacher inservice training (TRAINING)	-.00	.21	--		
4. School library (LIBRARY)	.02	.00	-.05	--	
5. Principal's education (PRINCIPAL)	-.20	-.17	.02	.01	--
Achievement					
6. Total achievement, fall year 1 (CTBS0)	.52	.34	.00	-.02	-.11
7. Total achievement, spring year 3 (CTBS3)	.43	.29	-.02	.01	-.15
Background Characteristics					
8. Father's education (FATHER)	.40	.17	-.10	.04	-.05
9. Mother's education (MOTHER)	.39	.22	-.08	.04	-.07
10. Occupation of household head (OCC)	.27	.15	-.04	-.00	-.05
11. Family income (INCOME)	.46	.35	-.08	.00	-.14
12. Race/ethnicity (RACE)	.51	.77	.16	.00	-.18
13. Presence of 2 parents (2PARENTS)	.26	.31	.01	-.00	-.16
14. Number of books in home (BOOKS)	.39	.32	.01	.02	-.10
15. Parents' attendance at school events (ATTEND)	.28	.19	-.04	.01	-.08

Note. — Each correlation is based on all cases in the three-year panel of cohorts 1 to 4 for which data are available for the pair of variables involved. The pooled within-cohort correlation is the pooled within-cohort covariance divided by the square root of the product of the appropriate pooled within-cohort variances. The pooled within-cohort variances and covariances are unweighted (i.e., simple) averages over the cohorts of the covariances and variances within each cohort.

than apparent at the school level, must be measured at the individual level as well in order accurately to be compared with background factors. Our results on background and schooling influences when individual-level data on schooling are available in fact support this supposition, as reported under the I composite below.

All of this evidence does not mean that school-level factors are completely ineffective. The addition to adjusted R^2 for spring 1979 achievement due to the 20 school variables considered after cohort, fall 1976 achievement~~,~~ and the eight components of the B composite (.006), compares favorably with the addition due to the B components after cohort, fall 1976 achievement~~,~~ and the school variables (.009). (Although not all of the 12 background variables considered are included in this comparison, Table 2-3 shows that the remaining background variables add less than .001 to adjusted R^2.) Thus, each set of variables contributes to the explanation of achievement gains net of the other. Though background is clearly more important, it seems unlikely that the net contribution of school factors, which is due to components completely uncorrelated with the former, simply represents additional unmeasured background characteristics.

If the influence of school factors is appreciable, can we say anything about the particular factors that contribute to achievement gains? Stepwise procedures have a notorious reputation for being 'blindly empirical' in the way the predictors are selected. In the present context, this reputation seems deserved; in other words, we do not believe much can be made of the particular school components selected or of their order of inclusion.

Two of the school characteristics that describe aspects of the school other than the peer environment (PRINCIPAL, TRAINING) were entered over all the other such characteristics, but are in general negatively related to the socioeconomic background and achievement of the student (Table 2-5) and exhibit relatively strong *negative* effects on achievement gains (Table 2-9). Such effects are implausible. Nevertheless, the two characteristics may negatively represent some aspect of the school environment or program, ~~albeit negatively,~~ which does affect achievement in a positive manner. Since the composite itself, rather than the individual components, is ~~are~~ of interest, these two characteristics were retained despite their uninterpretable behavior.

The significance of the school context variables--SCH-CTBS and SCH-RACE--in the S composite, is of greater interest because of earlier suggestions (Coleman et al., 1966: ch. 3) that the peer environment is an important influence on academic achievement and indeed is more important than the character of the school itself. Evidence for this proposition within our data is weak. Consider, ~~first,~~ the prediction of the student's initial achievement (CTBS0) by school characteristics. The school average achievement at the same point in time (SCH-CTBS) is by far the single most important predictor (Table 2-5) and indeed is more or less sufficient by itself (Table 2-4). Part of this sufficiency is due to the fact that the correlation between the two achievement measures is inflated by the inclusion of the student's own achievement in his grade's school average. But the bulk of this relationship is due to the assortment of students among schools by achievement and background characteristics. The average achievement of the student's grademates alone tells us as much about the differences between schools as the does the identity of the schools themselves (excluding cohort-by-school interactions), in terms of accounting for the student's own achievement. In addition, the average achievement of the student's school is highly correlated with his own socioeconomic background (Table 2-5). Thus, when we examine the static relationship between the student's own achievement and that of his grade and school, much of that association is due to the fact that his peers' status is a proxy for the student's own background.

Of course, there may still be a residual influence of school context after background and other school characteristics are controlled. Since such influence with respect to achievement *gains* is of primary interest, consider the prediction of achievement in the spring of 1979 (CTBS3), net of fall 1976 achievement (CTBS0) and cohort. It has been suggested (Coleman et al., 1966: ch. 3) that the racial mix of the student body is of major importance among school factors in accounting for school-to-school differences in achievement. While our stepwise procedures without consideration of background factors appear to support this suggestion, once those factors are taken into account, SCH-RACE contributes virtually nothing. Thus, there is no evidence in our data for the influence of racial composition of the student body that cannot more plausibly be accounted for by initial achievement and background alone.

On the other hand, the school average achievement explains a small additional (adjusted) proportion of the variance (.002) in spring 1979 achievement net of cohort, initial achievement, background, and the other 19 school characteristics considered. However, this increment is still substantially smaller than the increment due to the noncontextual school characteristics after cohort, initial achievement, background, SCH-RACE, and SCH-CTBS are taken into account (.009). Thus, our data provide no confirmation of the greater importance of contextual factors.

Moreover, one can always attribute the residual influence of context to unmeasured aspects of initial achievement, background, and school characteristics other than ~~the~~ peer environment. Hence, while the small effect of SCH-CTBS is suggestive of some peer influences within the school environment, it is by no means demonstrative. Other studies have indicated effects of peer socioeconomic status, academic ability, and behavior on educational and/or occupational aspirations, educational attainment, and academic performance relative to that of peers as measured by grades or class rank (Alexander and Eckland, 1975; Alwin and Otto, 1977; Campbell and Alexander, 1965; Duncan et al., 1968), but there is none of which we are aware offering corroborative evidence of peer influences on academic achievement as measured by standardized tests. It seems reasonable to suppose that such effects are likely to exist with respect to academic achievement as well, but that they are small--at least at the school level--as in other areas of achievement (Hauser et al., 1976).

Finally, we note for future reference that whatever components underlie the effects of schools on achievement, it is precisely because of the factorial complexity of the S composite, particularly as a surrogate for neighborhood and background, that it appears as an exogenous factor in our models and is separated from individual- and classroom-level educational experiences. This complexity enables us to treat the composite as a measure of school characteristics or as another measure of background when decomposing the explained variance in later achievement and individual school experiences or when evaluating the total effects of the various exogenous factors on them ~~the same~~.

The X (School-Experience) Composite

The Components. The X composite consists of nine components selected from a set of 12 chosen to represent school experience:

(1) The average academic achievement of the student's homeroom (HR-CTBS),

(2) The racial composition of the homeroom (HR-RACE),

(3) The average teaching experience of the reading and math teacher in years (TCHR-EXP),

(4) An indicator of whether or not the student received compensatory educational services in reading and/or math (CE),

(5) Attendance in weeks during the school year (WEEKS),

(6) The hours of regular instruction in reading received by the student during the school year (REG-READ),

(7) The hours of special instruction in reading (SPL-READ),

(8) The hours of regular instruction in math (REG-MATH), and

(9) The hours of special instruction in math (SPL-MATH).

The remaining three school-experience variables considered were:

(1) The student's reading and math teachers' average education,

(2) Their average extent of recent inservice training in their areas of specialization, and

(3) Their average number of recent college courses in their areas of specialization.

The instructional hours were obtained from teacher reports of the student's: (1) attendance in reading or math classes of varying sizes, instructed by the (regular) 'classroom' teacher or a 'special' reading or math teacher, (2) receipt of assistance from teacher aides, other students, or adult volunteers, and (3) independent seat work. (The forms on which the teachers reported a student's time in instruction or independent learning did not define the terms 'classroom' [hereinafter, 'regular'] and 'special' teacher or distinguish 'regular' from 'special' independent learning. Presumably, special teachers could have taught both advanced and disadvantaged students., On balance, however, special instruction was given more to disadvantaged students [Table 2-8 below].)

Actually, the forms for reporting instructional hours contained 10 categories of instruction among the three areas. Wang et al., (1981: ch. 3) collapsed these categories into three--regular instruction, special instruction, and tutorial/independent work--and found no significant difference in the prediction of achievement using their three categories instead of all 10. For simplicity, we further distributed the number of hours spent in tutorial/independent work equally between regular and special instruction and retained only the latter two categories.

The Stepwise Procedure. Because ~~Since~~ data on the X components were available on a yearly basis, the composite was formed for each of the three study years, although the components and their weights are identical for each year.

In selecting the components for the composite, statistical significance at conventional levels was used as the criterion, as previously noted. See Table 2-6 . It turned out that an α level of .10, .05, or .01 would have made no difference in the results. Although two of the four classroom-exposure variables (REG-READ, SPL-READ, REG-MATH, and SPL-MATH)--one in reading and the other in math--were not significant, all four were retained because of the integral relationships between regular and special classes. Therefore, ~~The outcome was the selection of the~~ the nine components of the composite were selected.

With individual- and classroom-level data on schooling, we can again address the same issues that were raised in considering the S composite. Here, however, we consider the prediction of the spring test at the end of a school year, given the spring measurement of the previous year (or the fall measurement, in the case of the first study year).

Table 2-6

Stepwise Regressions for the School-Experience Composite With Spring 1977 Achievement as the Criterion

Step	Component Entered	Adjusted R^2
1.	Fall 1976 achievement (CTBS0) and cohort	.900
2.	Receipt of compensatory education (CE)	.901
3.	Attendance in weeks during year (WEEKS)	.902
4.	Student's reading and math teacher's teaching experience (TCHR-EXP)	.902
5.	Average achievement in student's homeroom (HR-CTBS)	.902
6.	Hours of special reading instruction received during year (SPL-READ)	.902
7.	Racial composition of student's homeroom (HR-RACE, replacing HR-CTBS)	.902
8.	HR-CTBS (re-entered)	.902
9.	Hours of regular math instruction received during year (REG-MATH)	.903
10.	Remaining 3 school variables	.903

Note. — The stepwise procedure is based on the unweighted cases in the one-year panel of cohorts 1 to 6 for which data are available on the criterion and all the predictors considered (N=14,442).

Table 2-7 exhibits the proportion of the residual variance in the spring achievement scores of each year, net of initial achievement and cohort, that is accounted for by all the components of the B and X composites and by various subsets of these. (In this table, 'schooling' refers only to the factors of the X composite.) For those accustomed to the coefficients of determination of achievement in cross-sectional data, the proportions of residual variance in later achievement explained by background and schooling may appear small, but they are nonetheless appreciable, ranging from four to seven percent. (The proportions would be larger if we had not also removed initial achievement and cohort from the background and schooling components as well.)

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Table 2-7

Proportion of Residual Variance in Later Achievement, Net of Initial Achievement and Cohort, Explained by Background and Schooling Factors and Various Components of Schooling for the Three Study Years

Variables	Study Year		
	1	2	3
All background and schooling components	.041	.056	.070
Background	.027	.034	.036
Schooling	.022	.034	.043
Background, net of schooling	.019	.022	.027
Schooling, net of background	.014	.022	.033
Classroom context, net of background	.000	.005	.008
Classroom context, net of all other schooling components and background	.000	.004	.009
Quantity of instruction, net of background	.007	.008	.011
Quantity of instruction, net of all other schooling components and background	.005	.006	.011

Note. — For each of the years, later achievement is the spring achievement score for the year. For the later two years, initial achievement is the spring score of the previous year, while for year 1, it is the fall score of the same year.

The table shows that the influences of background and schooling, though somewhat varying over the three years, are relatively equal on balance, whether we consider the addition due to background or schooling alone or the addition of each after the other is already taken into account. The fact that there is both a substantial reduction in the contribution of either factor when it is considered after the other and a substantial contribution of each factor net of the other, indicates that the two factors are interrelated, yet have distinct, direct effects. In particular, considering the number of background indicators employed and the little addition to R^2 likely to come from inclusion of other such indicators, the schooling contribution is appreciable and probably robust (in a statistical sense). Thus, with schooling experiences measured within schools--at the levels of individuals and classrooms--and with longitudinal data on achievement, we have some palpable evidence that schooling makes a difference. In particular, it appears that, averaged over the elementary grades, differential educational experiences of students within and among schools have as much impact on ~~the~~ development of the students' academic skills in any one school year as do their ~~that of the~~ homes ~~from which they come~~. Later, we will find that schooling influences are particularly strong in the earliest years and diminish rather rapidly as the child progresses through school (see Chapters 3 and 4). In short, the relative effects of background and schooling depend on grade level, but the effects of both averaged over the elementary education period are clearly significant.

For some time now, we have known that there are substantial differences in adult socioeconomic achievement between those who complete more and those who complete fewer years of school (Blau and Duncan, 1967; Jencks et al., 1972). Common sense tells us that schooling also makes a difference in academic achievement insofar as some are denied (for whatever reason) opportunities to attend school. Several earlier studies (reviewed briefly in Jencks et al., 1972: 81-89) provide some evidence consistent with this notion. In particular, when schools have been closed for periods of time because of extraordinary events (e.g., a teachers' strike), achievement scores have suffered. In addition, Heyns (1978) has found greater gains in reading achievement during the school year than during the summer period for elementary students. Our current results suggest in addition that, among students who do attend school, their various educational experiences produce some variation in academic skills.

Perhaps of even greater interest than the relative influences of background and schooling is what the results suggest about the roles of classroom instruction and peer influences on the student's academic development. Wiley (1976) proposed that the quantity of classroom exposure has an appreciable impact on academic achievement, but Karweit's (1976) extensive reanalysis of the EEO data, on which Wiley based his conclusions, failed to confirm his results. Our earlier evidence regarding the effect of peer characteristics averaged by school was that such effects were small and in fact not at all the primary aspect of school influences. This leaves the question whether the characteristics of students in the smaller and more homogeneous classrooms--within which there is presumably greater interaction among students--would better account for the variance in their achievements.

Table 2-7 displays the additional proportion of variance in academic growth explained when the classroom context (HR-RACE, HR-CTBS) and the quantity of instruction received (WEEKS, REG-READ, SPL-READ, REG-MATH, SPL-MATH) are considered after background, or after background and the remaining school-experience variables. Clearly, the influence of separate components of schooling can be quite small, but there is an appreciable influence of quantity of instruction in each of the years, consistent with Wiley's suggestion. Although there is some influence of classroom context in two of the three years, such influence is dominated by that of instruction and thus is not at all the single most important school influence, as previously suggested (Coleman et al., 1966: ch. 3). (Of course, the classroom context is measured at the classroom-level while the hours of instruction received are measured at the individual-level, but we have no better means of representing peer influences within this study. Aggregation of the instructional hours to the class-level would alter the meaning of those variables.)

Table 2-8 gives some indication of the relationship between the school-experience variables on the one hand and background and achievement on the other and ~~that~~ may explain ~~why~~ the observed difference~~s~~ in the influences of context and instruction ~~occur~~. Although the characteristics of the student's homeroom are highly correlated with his achievement, they are also highly correlated with his background. The remaining schooling variables are less strongly related to achievement, but are also more independent of

Table 2-8

Pooled Within-Cohort Correlations of School Experiences, Achievement, and Background for Cohorts 1 to 6

Variables	1	2	3	4	5	6	7	8	9
1. Average achievement in homeroom, fall year 1 (HR-CTBS)	--								
2. Proportion white or Asian in homeroom, year 1 (HR-RACE)	.60	--							
3. Average experience of teachers (TCHR-EXP)	.13	.08	--						
4. Participation in compensatory education program(s) (CE)	-.27	-.27	-.10	--					
5. Attendance in weeks (WEEKS)	.19	.02	.03	-.10	--				
6. Hours of regular reading instruction attended (REG-READ)	.03	.01	.09	-.17	.15	--			
7. Hours of special reading instruction attended (SPL-READ)	-.12	-.08	-.10	.27	.10	-.35	--		
8. Hours of regular math instruction attended (REG-MATH)	-.01	-.02	.04	-.11	.12	.43	-.15	--	
9. Hours of special math instruction attended (SPL-MATH)	-.13	-.13	-.09	.22	.09	-.16	.43	-.38	--
10. Total achievement, fall year 3 (CTBS0)	.56	.35	.08	-.34	.15	.10	-.21	.06	-.15
11. Total achievement, spring year 3 (CTBS1)	.51	.34	.11	-.35	.16	.11	-.22	.07	-.15
12. Father's education (FATHER)	.36	.20	.08	-.23	.18	.07	-.11	.00	-.07
13. Mother's education (MOTHER)	.38	.26	.08	-.23	.20	.07	-.10	.02	-.07
14. Occupation of household head (OCC)	.27	.18	.06	-.16	.11	.02	-.05	.01	-.06
15. Family income (INCOME)	.42	.37	.07	-.21	.19	.03	-.08	-.01	-.08
16. Race/ethnicity (RACE)	.47	.77	.07	-.25	.02	.03	-.10	-.00	-.13
17. Presence of 2 parents (2PARENTS)	.23	.31	.04	-.09	.08	-.01	-.03	.01	-.05
18. Number of books in home (BOOKS)	.39	.35	.05	-.22	.12	.04	-.10	-.01	-.08
19. Parents' attendance at school events (ATTEND)	.22	.14	.04	-.10	.17	.06	-.05	.01	-.05

Note. — Each correlation is based on all cases in the cross-section sample for which data are available for the pair of variables involved.

background. While there is some indication that compensatory educational services and special reading and math instruction are received more by academically and socioeconomically disadvantaged students, the relatively greater independence of the noncontextual schooling variables from background probably accounts for their stronger residual influence.

The only fact marring the present results on the influence of classroom hours is that the effect of special reading instruction is negative, Table 2-9 . It is unlikely that this service actually reduces achievement. Rather, it more likely reflects differences in other characteristics associated with selection for this service and not sufficiently taken into account by the variables employed. Once admitting this possibility, we must also admit that these uncontrolled characteristics may reflect the academic and social backgrounds of the students rather than receipt of other, unmeasured services that have a positive impact on achievement and that are negatively related to receipt of special reading instruction.

A countervailing consideration, however, is ~~that~~ the positive effect of the total configuration of classroom hours for any given student ~~is positive~~. To be sure, the negative coefficient for SPL-READ in the X composite means that it is possible to obtain a negative overall number of (weighted) classroom hours. Indeed, the portion of the X composite that is due solely to the four hours variables ~~alone~~ is negative for between .7 and 1 percent of the sample in each of the three years. However, this is an extremely small percentage of the cases. Thus, it makes sense to treat the

Table 2-9

Raw and Standardized Coefficients of the Components in the Background, School-Characteristics, and School-Experience Composites

Components	Coefficients	
	Raw	Standardized
Background Characteristics		
Father's education (FATHER)	4.408	.108
Mother's education (MOTHER)	4.264	.072
Occupation of household head (OCC)	-.341	-.007
Family income (INCOME)	-.565	-.029
Race/ethnicity (RACE)	5.541	.035
Presence of 1 parent (1PARENT)	7.816	.041
Number of books in home (BOOKS)	2.055	.059
Parents' attendance at school events (ATTEND)	.160	.001
School Characteristics		
Average achievement in student's school and grade (SCH-CTBS)	.012	.005
Racial composition of student's school and grade (SCH-RACE)	3.776	.018
Teacher inservice training (TRAINING)	-4.214	-.028
Principal's education (PRINCIPAL)	-20.511	-.057
School library (LIBRARY)	12.449	.020
School Experiences		
Average achievement in student's homeroom (HR-CTBS)	.077	.030
Racial composition of student's homeroom (HR-RACE)	2.398	.043
Student's reading and math teachers' teaching experience (TCH-EXP)	.263	.013
Receipt of compensatory education (CE)	-4.760	-.053
Attendance in weeks during year (WEEKS)	.667	.024
Hours of regular reading instruction received during year (REG-READ)	.012	.014
Hours of special reading instruction received during year (SPL-READ)	-.020	-.020
Hours of regular math instruction received during year (REG-MATH)	.024	.018
Hours of special math instruction received during year (SPL-MATH)	.043	.026

Note. — The composite coefficients are based on the pooled within-cohort correlations in Table 2-1 and Table 2-7, except that here father's education or mother's education is coded "0" for the purpose of computing the coefficients if he or she is absent from the home. Also, a dummy variable representing the presence of only one parent is used instead of a dummy variable representing the presence of both parents, as in the earlier tables.

overall coefficient for the total weighted hours within the X composite as positive, since if we considered it as negative, we would have to consider the weighted hours to be negative for over 99 percent of the cases! (The weighted sum $\sum_{i=1}^{4} a_i x_i$ can be written as $c \sum_{i=1}^{4} b_i x_i$ where x_i represents one of the four classroom hours variables, $cb_i = a_i$ for every i, and c is an arbitrary constant in sign and magnitude. If we took the effect c of the weighted sum $\sum_{i=1}^{4} b_i x_i$ to be negative, then the sum itself would be negative for almost all cases.)

Finally, as noted in Table 2-9, the coefficient for CE is negative and is ~~the~~ largest in magnitude when the components are standardized. As in the case of special reading instruction, the sign of the coefficient probably reflects aspects of initial achievement and background ~~that are~~ not sufficiently taken into account. This circumstance, together with the size of the contribution of the CE component, may raise ~~the~~ concern that the effects of the school-experience composite on later achievement, as reported in Chapters 3 and 4, may be overestimated, particularly for cohort 1, which exhibits the strongest effects. However, exclusion of the CE component made little difference; the two composites--with and without CE--were so highly correlated that ~~the~~ differences in the effects for cohort 1 were inconsequential.

Component Weights for the Three Composites

Table 2-9 reports the raw (metric) and standardized coefficients for the components in each composite. The standardized coefficients were obtained by using the pooled within-cohort variances.

For the B and S composites, stet ~~the~~ variances of the components and criterion were computed as simple averages of the cohort variances across the four cohorts in the three-year panel. For the X composite, stet ~~the~~ variances of the components and criterion were computed as weighted averages of the cohort variances for each year and cohort for which data were available over the three years of the study. Since three years of data were available for grades 3 to 6 while only one and two years of data were available for grades 1 and 2, respectively, the covariances for the latter were weighted by 3 and 1.5, respectively, while the remaining covariances were equally weighted.

(Sample weights were not computed for the two-year panel. Although such weights could have been computed as they were for the cross-section and three-year panel, the means and covariances for the second year were computed for the weighted three-year panel, except for cohort 5. In this case, ~~the~~ statistics were computed on the unweighted two-year panel for simplicity.)

The magnitudes of the standardized coefficients give some sense of the relative weights of the components in each composite. For example, a standard deviation increase in BOOKS has a greater effect on CTBS3 (all other things being equal) than does a standard deviation increase in INCOME.

For reasons already given, it would not be productive to attempt a substantive interpretation of the weights. The weights simply define composites that are predictive of achievement gains and that presumably reflect the factors of interest. Moreover, standardized coefficients are not very meaningful for dichotomous variables (e.g., RACE, CE) and the FATHER, MOTHER, and 1PARENT variables are integrally related and meaningful only in their natural (raw) units. However, even the metric coefficients are in some cases suspect. For example, these coefficients imply that a child's achievement will be greater if he has only one parent than if the missing parent were present and had attained only eight or fewer years of schooling ~~(or less)~~. This eventuality seems unlikely. In addition, not much can be made of the negative coefficients for some components. We have already noted the reason that some of the components of the X composite have negative coefficients. In addition, there are negative coefficients for some of the B components, reflecting 'suppressor' effects (Lord and Novick, 1968: 271-2). Here we have an example of an oft-occurring result in regression analyses: when a number of positively and substantially related predictors are included in a regression, the signs of the coefficients of some of the weaker predictors, which would be positive if the stronger predictors were excluded, become negative. If the components with the negative coefficients were excluded, the other, positive coefficients would generally decrease. The result is that since INCOME is positively related to BOOKS, the increase in the B composite due to an increase in BOOKS tends to be partially offset by an increase in INCOME as well. If INCOME were excluded from the B composite, the effect of BOOKS would decline. One could omit the suppressor variables from the composites, but at some loss in accuracy of prediction (R^2) of (later) achievement.

Finally, we note that the large negative coefficients in the S composite led to S scores that were on the average negative. This result poses no substantive problems, ~~since~~ because the mean of the variable has no effect on its path coefficient, whether metric or standardized.

Conclusions

~~In the context of~~ While constructing three composites to represent background and school factors in models of the schooling process, we found that, although the underlying components of the factors are difficult to disentangle, both factors appear to contribute to achievement gains. The most important characteristic of the home environment that appears to produce achievement gains, is the kind of educational support and encouragement the child receives. At school, the child's sheer quantity of exposure and attention to instruction (assuming a proportionate relation between hours attended and hours attended to) appears to produce achievement gains. We found no support for the proposition that the peer environment at school is the most important school influence. Indeed, the effects of the peer environment measured at the school-level are weak, and those measured at the classroom-level are not consistent over the three years of the study. Finally, we found that while background factors are significantly more important ~~in comparison to~~ than school-level factors in the explanation of achievement gains, once classroom- and individual-level data on school experiences are taken into account, the direct influences of schooling and background (averaged over the elementary grades) are substantially similar in magnitude within any given year.

In the next two chapters, we use the three school and background composites in models that ~~will~~ not only provide an additional assessment of the relative importance of schooling and background, but also indicate how these affect achievement over time. The appreciable direct effects of schooling on achievement found in this chapter, must be reconciled with the perpetuation of the background-achievement relationship ~~that is~~ established prior to school. We also ~~will~~ examine the parts of the schooling process that account for ~~the~~ coexistence of these two results.

CHAPTER 3. THE SCHOOLING-ACHIEVEMENT PROCESS

A basic structural-equation model of the schooling process indicates that educational resources are distributed to students primarily on the basis of academic skills and STET ~~the~~ school attended rather than social origins. To ~~be~~ sure, there is a significant amount of preferential treatment accorded to children of privileged backgrounds above that which they would receive if the school they attended depended solely on their abilities and if the resources they received depended solely on their school and abilities. Yet there are important opportunities for receiving school resources independent of social background. Despite these opportunities, however, the strong association between background and abilities established prior to schooling, in combination with a significant degree of preferential treatment accorded to socioeconomically privileged children, results in a substantial association between schooling and background. Moreover, while schools exert a tangible influence on achievement, the effects decline rapidly and markedly with increasing grade. The decline is particularly drastic for reading achievement, with no evidence of schooling effects in the later grades.

A fundamental observation to be explained by any model of the schooling process is that the correlation between background and achievement remains essentially constant throughout the elementary years, although the association is stronger in reading than in math. This perpetuation of the background-achievement relationship is the result of (1) the strong, combined effect of highly correlated background and ability factors, (2) the favorable distribution of school resources to children of privileged backgrounds (which, of course, is the very phenomenon that compensatory programs attempt to mitigate), and (3) the generally modest effect of schooling on achievement. Even if the preferential treatment accorded to socioeconomically privileged students were completely eliminated, the impact on the background-achievement relationship would be insignificant because of (1) the modest effects of schools, (2) some remaining association between background and schooling due to the correlation of each with initial achievement, and (3) the predominant importance of earlier achievement in the determination of later academic success.

In this chapter, we describe a structural-equation system or 'path' model that formalizes our causal assumptions about the schooling process as described in Chapter 1. We then examine how background and schooling influence achievement over three years among the four cohorts observed. Next, we discuss how educational opportunities may be assessed in terms of the model. Finally, we consider the differential influences of background and schooling on reading and math achievement.

The Basic Model

Our basic model of the schooling process is depicted in the form of a path diagram, Figure 3-1. This model, elaborated in various forms, lies at the heart of the analyses throughout this study.

The diagram suggests that the child's social and academic origins affect his schooling, which in turn affects his achievement. The effects of origins are presumed to be both direct and indirect (through schooling).

Further, the model assumes a correlation between initial achievement and background factors (represented by a curved, double-headed arrow between the two) rather than a causal dependence of the former on the latter. This is done, first, because some background factors--for example, parents' interest in the child's achievements and provision of educative materials--may be a consequence as well as a determinant of achievement to that point, especially in later years of his

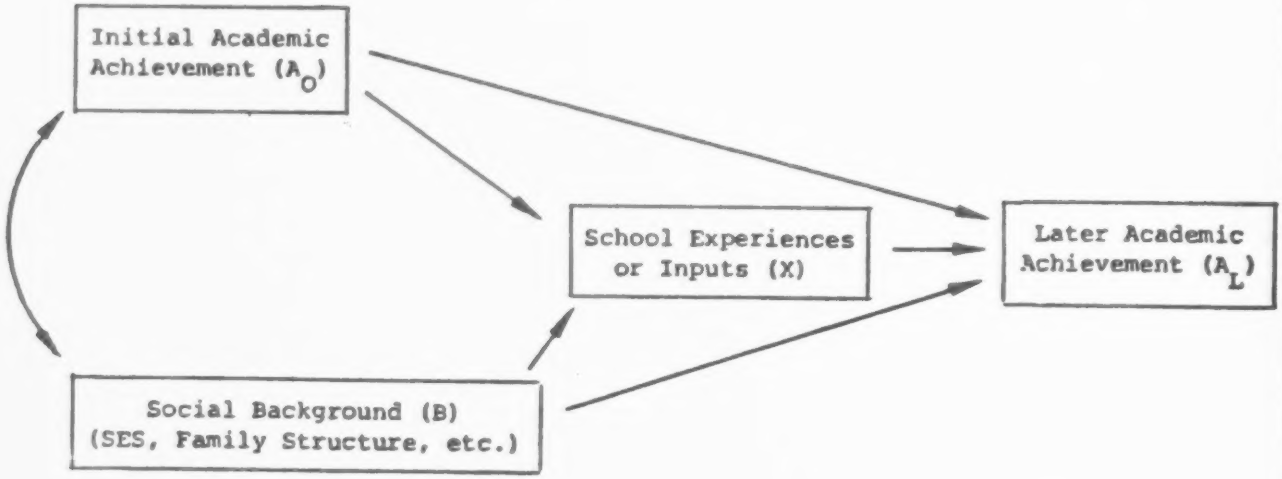
Initial Academic Achievement (A_O)
School Experiences or Inputs (X)
Later Academic Achievement (A_L)
Social Background (B) (SES, Family Structure, etc.)

Figure 3-1

A Basic Model of the Schooling Process

schooling. Second, both the parents' socioeconomic status and the child's own achievement may depend on prior factors, such as inherited intelligence. In short, the association between initial achievement and background is unlikely to be due simply to the dependence of the former on the latter. Consequently, the association is left unexplained in the model by postulating both factors to be 'exogenous', as further explained below.

Statistically, the model may be written out as a system of two 'structural' equations, one for school experiences (X) and the second for later achievement (A_L):

$$X = \beta_X + \beta_{XA_O}A_O + \beta_{XB}B + \epsilon_X \tag{3.1}$$
$$A_L = \beta_{A_L} + \beta_{A_LX}X + \beta_{A_LA_O}A_O + \beta_{A_LB}B + \epsilon_{A_L}.$$

Each equation is a regression equation ~~with~~ including as regressors those variables having direct effects on the variable of interest. ~~included as regressors,~~ Academic and social background (A_O, B) are 'exogenous' variables because their values are determined 'outside the system', that is, in a way unexplained by the system and taken as given, whereas the other, 'endogenous' variables (X, A_L) are determined by the exogenous variables and the 'disturbances' (ϵ_X, ϵ_{A_L}) in the way described by the system, as specified by the βs.

In the nomenclature of structural-equation modeling, the variable of interest whose behavior is explained by a given equation is the 'current endogenous' variable, while the regressors are 'predetermined' variables, which may include other endogenous variables as well as exogenous variables, as in the equation for A_L. The βs are 'structural coefficients', 'path coefficients' (particularly when the variables are standardized), or simply '(direct) effects.'

The system of equations (3.1) provides an underlying 'causal' structure that explains the covariances among the endogenous variables. For example, if the disturbances are uncorrelated with one another and with the random exogenous variables, we can see, by ~~from~~ substituting the right hand side of the first equation for the variable X in the second equation, that the covariance between A_L and X will be a function of the structural coefficients and the variances and covariance of the two exogenous variables. In a similar manner, the model also explains stet the covariance relations between the endogenous variables and the random exogenous variables. The covariance structure of the random exogenous variables themselves is taken as given and left unexplained.

The model is not subject to test as it stands because it perfectly accounts for the covariance structure of the variables. Its purpose is to provide an *interpretation* of the relationships observed on the *assumption* that the covariance structure arises from the model. This process is the inverse of ~~that of~~ using the observed relationships (the covariance structure) to test the plausibility of the posited model. Assuming the model is true, however, one can test various special cases that constrain certain effects. We shall refer to the full underlying model (which is not subject to test) as 'saturated' and to any model derived therefrom that places certain restrictions on the structural parameters and thereby allows a test, as 'unsaturated'.

Maximum-likelihood estimation of the effects in the model and likelihood-ratio hypothesis testing under the assumption that the disturbances and random exogenous variables are independent across observations and normally distributed are well established (Land, 1973; Theil, 1971: 460-1, 524-5; Jöreskog, 1973, 1977; Jöreskog and Sörbom, 1978). These procedures are employed as the basis for statistical inferences throughout. Further details about the procedures are provided as particular models are discussed.

The basic model of Figure 3-1 provides a means for a simple first assessment of the extent to which schooling exerts an independent or compensating influence on achievement growth. With only two measurements of achievement and a single period of schooling between measurements, the causal scheme is straightforward. If the direct effects of background and initial achievement on later achievement decrease and if school experiences become increasingly important and independent of background over time, use of the maximum observed interval between achievement measurements should cast school factors in the most favorable light. In addition, by employing the maximum test-retest interval, we allow the true change in achievement scores and the variance in intervening influences to be maximal, so that pretest is less likely to account for most of the variance in posttest simply because of test reliability or short-term trait stability.

If schools exert a significant *independent* influence on academic achievement, then we should find that the total resource exposure over the three-year period has substantial direct consequences for achievement in spring 1979, both in absolute terms and relative to the direct and indirect influences of background (unless our measures of school experiences are inadequate or off-target). Resource exposure should also exhibit substantial independence of background.

Modification of the Basic Model

The actual models used in ~~the~~ assess~~ment of~~ing school-and background influences involve a slight modification of the model in Figure 3-1, motivated by two considerations. First, school characteristics were essentially constant over the three years of the study. Second, and more important, in the absence of adequate controls for neighborhood and community characteristics, school characteristics may be their surrogates for ~~them.~~ Thus, aggregate and global characteristics of the schools are treated as exogenous.

Once school characteristics are controlled, other schooling variables such as classroom characteristics and individual educational experiences can more safely be attributed to schooling entirely. ~~Since~~ Because school experiences are endogenous, effects of aggregate and global classroom characteristics were combined with individual experiences (as described in Chapter 2) to obtain a single school-experience composite for the sake of model simplicity.

In Chapter 1, we also mentioned that one direction of elaborating the basic model was to consider initial level of academic motivation as another exogenous variable and subsequent levels as endogenous. Two indicators of initial motivation seemed promising and were considered: the number of books brought home by the child from the library and the number of hours he spent reading during an average weekday of the school year (both as reported by parents). Preliminary results showed, however, that their effects were generally small once background and initial achievement were taken into account. Thus the simpler models without measures of initial motivation were employed.

Our starting point, then, is the (saturated) model:

$$(3.2)\quad \begin{aligned} X &= \pi_{XA_0}A_0 + \pi_{XS}S + \pi_{XB}B + \varepsilon_X \\ A_3 &= \pi_{A_3X}X + \pi_{A_3A_0}A_0 + \pi_{A_3S}S + \pi_{A_3B}B + \varepsilon_{A_3}, \end{aligned}$$

where S represents school characteristics and A_L has been replaced by A_3, the spring 1979 achievement score. The variables are now centered (i.e., deviated from their means), so that the constant ('intercept') terms are absent. In addition, we generally consider the variables as standardized, and hence the πs as standardized coefficients, unless otherwise noted. The model is depicted in the path diagram for cohort 2 in Figure 3-2, the diagrams for the other cohorts representing unsaturated special cases.

Before interpreting this model for the four cohorts, there are a few additional technical details to observe. B and S are the composites, respectively, for background and school characteristics assessed in the first study year. X is the total of individual- and class-level educational experiences over the three years. A_0 is fall 1976 achievement (CTBS0) and A_3 spring 1979 achievement (CTBS3).

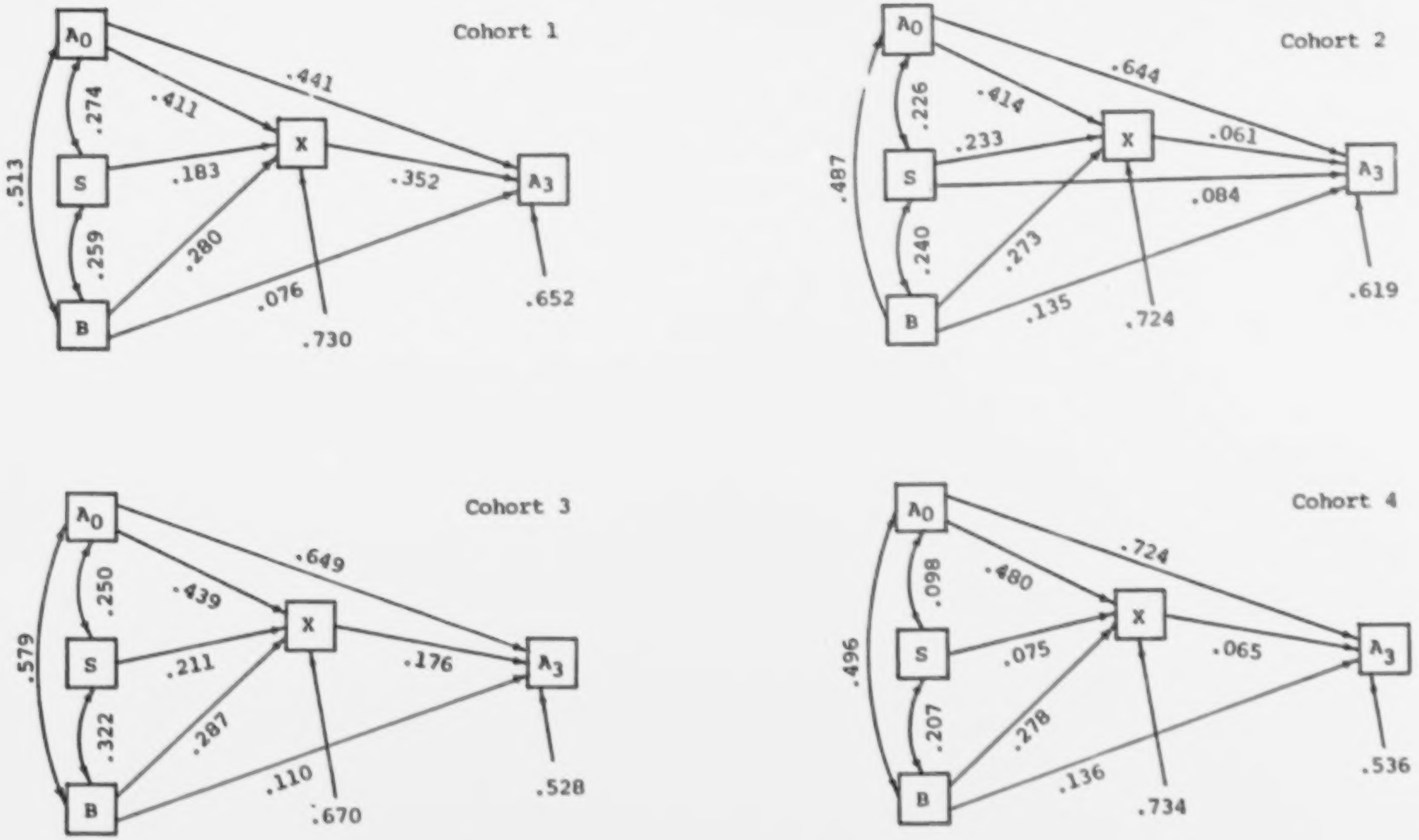
Cohort 1
A0
S
B
X
A3
.274
.259
.513
.411
.441
.183
.280
.076
.352
.730
.652
Cohort 2
A0
S
B
X
A3
.226
.240
.487
.414
.644
.233
.061
.084
.273
.135
.724
.619
Cohort 3
A0
S
B
X
A3
.250
.322
.579
.439
.649
.211
.287
.110
.176
.670
.528
Cohort 4
A0
S
B
X
A3
.098
.207
.496
.480
.724
.075
.065
.278
.136
.734
.536

Figure 3-2

Path Diagrams With Estimates of Standardized Coefficients for the Model of Equation 3.2, With Nonsignificant Effects Omitted

Models of the form in Eq. (3.2) with uncorrelated disturbances and 'one-way causation' (e.g., X affects A_3, but A_3 does not affect X) are called 'recursive'. For such models, whether saturated or unsaturated, the maximum-likelihood estimates under the normality assumptions described above are obtained by ordinary least squares (OLS). (The OLS estimates have other desirable properties without the assumption of normality of the disturbances or of the random exogenous variables. However, we will require the assumption of normality in any event in order to use established hypothesis-testing procedures. See Land [1973] and Theil [1971: 460-1, 524-5].) ~~The~~ Estimates of the standardized effects are reported in Table 3-1 as well as in Figure 3-2. ~~The~~ Corresponding metric estimates are given in Table C-3 of Appendix C.

Table 3-1

Estimates of Standardized Effects for the Models of Figure 3-2

Effect	Endogenous Variable	Predetermined Variable: Three-Year Total School Experiences	Predetermined Variable: Total Achievement Fall Year 1	Predetermined Variable: School Characteristics	Predetermined Variable: Student Background
		Cohort 1			
Direct	Three-year total school experiences	---	.411	.183	.263
Direct	Total achievement, spring year 3	.352	.441	.000	.076
Indirect	Total achievement, spring year 3	---	.145	.064	.099
Total	Total achievement, spring year 3	.352	.585	.064	.174
		Cohort 2			
Direct	Three-year total school experiences	---	.414	.223	.273
Direct	Total achievement, spring year 3	.061	.644	.084	.135
Indirect	Total achievement, spring year 3	---	.025	.014	.017
Total	Total achievement, spring year 3	.061	.669	.098	.151
		Cohort 3			
Direct	Three-year total school experiences	---	.439	.211	.287
Direct	Total achievement, spring year 3	.176	.649	.000	.110
Indirect	Total achievement, spring year 3	---	.077	.037	.051
Total	Total achievement, spring year 3	.176	.726	.037	.161
		Cohort 4			
Direct	Three-year total school experiences	---	.480	.075	.278
Direct	Total achievement, spring year 3	.065	.724	.000	.136
Indirect	Total achievement, spring year 3	---	.031	.005	.018
Total	Total achievement, spring year 3	.065	.736	.005	.145

Note. — The estimate of an effect is zero (indicated as .000) when the estimate under the saturated model ~~was~~ is not significantly different from zero. In that case, the unsaturated model ~~was~~ is adopted, with the corresponding parameter assumed to be zero.

The dashes (---) indicate that the parameter is not defined under the saturated model.

The estimates shown in Figure 3-2 follow path-diagrammatic conventions. Estimated correlations between pairs of exogenous variables are placed adjacent to the curved, double-headed arrows connecting members of the pairs. Estimates of the standardized path coefficients corresponding to the πs of Eq. (3.2) are displayed alongside the single-headed arrows from one variable to another. Finally, the standard deviation of each disturbance, which in a recursive system is the square root of the coefficient of alienation for the endogenous variable, is shown at the head of an arrow leading from the disturbance to the variable. This standard deviation is also the effect of the disturbance when it is standardized.

The estimates shown in Figure 3-2 are those stet ~~that are~~ significantly different from zero at the .05 level. The only coefficients eliminated were the direct effects of S on A_3 for cohorts 1, 3, and 4. It turned out that, given the sample sizes, coefficients of magnitude less than .05 were nonsignificant.

The means, standard deviations, and correlations of the variables on which all estimates and tests in this and the following chapter are based are reported in Table C-2 of Appendix C. The statistics are given by cohort for the three-year panel of cohorts 1 to 4. To minimize ~~the~~ loss of data, each statistic was based on the maximum number of cases with nonmissing data. In particular, the correlation matrices contain elements that are based on different subsets of the sample. Thus, prior to analyses of the matrices for each problem, it was determined that they were well-behaved (i.e., positive-definite). For the purpose of hypothesis testing, the average number of cases available for each cohort was used (N = 815, 751, 720, and 523 for cohorts 1 to 4, respectively).

Background, Schooling, and Achievement

We began our analysis of the schooling-achievement process by treating the three years of the study as a single period--without consideration of intervening achievements and their relations to schooling. In this simplified setting, ~~the~~ stet common patterns across the cohorts and the uniqueness of experiences of cohort 1 are immediately apparent.

First, let us examine ~~the~~ stet relations among ~~the~~ stet exogenous variables representing the characteristics of students and their schools as they entered the three-year period. It is clear that at any given grade (among those observed), there is a strong relationship between the level of skills with which a student enters school ~~on the one hand~~ (A_0) and his social background (B) on ~~the other~~. As noted in Chapter 2 and affirmed by our composites, this relationship appears neither to decline nor to increase with progression through school.

The substantial correlation between background and initial achievement notwithstanding, achievement at any grade level is largely independent of background. Since the portion ~~proportion~~ of variance in initial achievement accounted for by background

100

rarely amounts to more than a third, ~~this means that~~ about two-thirds of the variance is completely unrelated to and inexplicable by background. (Even if we consider S ~~as~~ an indicator of background, the remaining proportion of variance in initial achievement is still about .6). Thus, insofar as schooling and later achievement depend on initial achievement primarily, there are opportunities both for background to influence achievement indirectly and for factors independent of background to advance a child's academic skills.

Just as the child's initial achievement is related to social origins, so ~~also is~~ the quality of his ~~the~~ school is related to his background and, to a somewhat lesser extent, to his academic abilities as well. For the later grades, stet the association between initial achievement and school characteristics could be due to effects of schools on achievement or of achievement on school selection. At the point of entry into school in the first grade, however, the quality of school that the child attends clearly is related to initial achievement. (the partial correlation between S and A is .17 for cohort 1).

This pattern of approximately equal correlations between school quality on the one hand and background and initial achievement on the other is also evident for cohorts 2 and 3. For cohort 4, the two correlations decrease, with a sharper decline for the correlation between school quality and initial achievement. We suspect that the reason for ~~this~~ these declines is that the S composite does not function ~~as well~~ stet for this cohort as an indicator of school quality. Note that the composite also does the poorest job of accounting for school experiences for cohort 4, and has the least variance in this cohort. In reviewing the components for the S composite, we observed that cohort 4 was the only one for which there was no school whose principal had a doctor's degree, while the distributions for the other school characteristics were essentially similar across the cohorts. ~~Because of~~ this occurrence~~,~~ artificially restricted the variability of this composite for cohort 4 ~~was artificially restricted~~ in the sample. Because ~~Since~~ the principal's education figures prominently in the S composite (Table 2-9), it is likely that it alone accounts for the discrepant results for cohort 4.

Turning to the effects of the three exogenous variables on the first step in the schooling-achievement process, namely, school experiences (X), we note that for all cohorts, initial achievement is the most important factor. Increases in school services are about one-and-one-half or more times more responsive to increases (in standard-deviation units) in academic ability than to increases of social status or the quality of the school. In addition, the quality of the school attended is generally an important factor in the kinds of educational services to which students are exposed, the exception for cohort 4 probably being unimportant as noted above. (Parents who work hard to place their children in certain schools are already convinced of this relationship.)

Nevertheless, even with academic abilities and school quality controlled, family background exerts a relatively strong direct effect on experiences at school. Thus, services are dependent on background not only indirectly, through the skills with which children begin school and the schools to which they are assigned, but also directly. For those interested in seeing school resources allocated on a basis (statistically) independent of socioeconomic status (which is a goal of legislation affecting school finances), this is not a welcome result. Moreover, even if the direct effect of background on school services were nullified (by allocating services without direct regard to background ~~directly~~), a substantial correlation between services and background would remain, as indicated by the difference $r_{XB} - p_{XB}$, where r_{XB} is the (estimated) correlation between X and B and p_{XB} is the (estimated, standardized, direct) effect of B on X. This difference is the (estimated) part of the correlation that is due other than to the direct effect of B on X. The difference ranges from .253 to .322 for the four cohorts (see rows 1 and 2 of Table 3-2, panel 1, below; row 2 is the correlation between B and X) and would probably be greater if better measures of school characteristics were employed, since p_{XB} would probably be less in that case. (The use of better measures of school characteristics would probably shift greater weight to the indirect determination of school experiences by background [through the quality of the school attended], but the resulting component of the association between X and B due to $p_{XB} + p_{XS}r_{SB}$, representing the influence of background on school experiences not due to the association of background with initial ability, would probably not be changed.)

It is worth repeating that while background is an important factor in the receipt of school services, ~~the~~ academic ability of the student is more important. Moreover, even if ability and the quality of the school were completely dependent on background, the receipt of school services would be to the same degree independent of background as it is dependent on it, in the sense that the proportion of variance in X accounted for by all three exogenous factors is about one-half, leaving one-half to be accounted for by other factors. Thus, while the educational system clearly favors the socioeconomically privileged beyond what would be expected because of their abilities and schools' characteristics, there is also a large element of 'randomization' of services, providing important opportunities for children to receive services independent of their social origins.

Finally, we consider the effects of all four predetermined variables on achievement in the spring of the third year. First, we need some definitions. For recursive models, the 'total' effect of a predetermined variable on an endogenous variable is the sum of its direct effect and all its indirect effects. Loosely speaking, an 'indirect' effect is the product of direct effects 'connecting' the predetermined variable to the endogenous variable via a particular combination of intervening variables, proceeding in a 'forward' direction. (The arrows then form a causal 'chain'.) For example, the single indirect effect of A_0 on A_3 is through (its effect on) X and is $\pi_{A_3X}\pi_{XA_0}$. The total effect of A_0 on A_3 is $\pi_{A_3A_0} + \pi_{A_3X}\pi_{XA_0}$ and represents the effect of A_0 on A_3 both directly and indirectly. Similar rules apply to the other two exogenous variables. The values of the total and indirect effects are interpreted in the same way as the value of the direct effect, whether standardized or not.

For three of the four cohorts, the effect of the quality of the school attended is indirect. That is to say, while the school conditions the kinds of educational experiences to which the student is exposed, once such experiences are taken into account, the school itself generally has no influence on achievement.

For all four cohorts, achievement three years later not surprisingly depends predominantly on the abilities with which students entered the study. This is evident not only from the pattern of direct effects, but also from the patterns of indirect and total effects for the three exogenous variables (Table 3-1). Much more interesting, however, is the pattern of generally increasing direct effects from ~~the~~ earlier to later achievement, from cohort 1 to cohort 4, with the largest jump occurring from cohort 1 to cohort 2. Cohort 1 also exhibits the largest effect of school experiences on (later) achievement both in absolute terms and relative to the effects of initial achievement and background. Finally, the variance of the disturbance (residual) for later achievement exhibits a general decline from cohorts 1 to 4.

These patterns indicate that as the child progresses through elementary school, his later achievement becomes increasingly predictable, less dependent on his school and school experiences, and more directly dependent on his earlier academic skills. The direct translation of social status and initial academic abilities into later achievements is less efficient in the early years of elementary schooling, but becomes more so in a short interval of time. While we cannot project the patterns beyond the elementary years, these results and those using the year-to-year achievement data in Chapter 4 suggest that schools become less influential and achievement increasingly, directly dependent on prior academic statuses, as the child progresses through school. The historic~~al~~ preference for targeting compensatory educational services primarily at the lower grades probably reflects in part educators' intuitive knowledge of this phenomenon.

Although the pattern of direct effects of school experiences (X) on later achievement is not as consistent as one would like to support this thesis, there does appear to be a declining influence of schooling on achievement over the cohorts if one includes the direct effect of the quality of the school (S) as well. In this sense, cohort 1 shows the strongest schooling influences, while cohort 4 shows the weakest. We will encounter further evidence in support of this conclusion when we consider the effects of schooling independent of background in the next section of this chapter, and also when we include the intervening achievement levels between fall 1976 and spring 1979 and disaggregate the school experiences by year in Chapter 4.

There are two possible alternative explanations for the observed declining influence of schooling. One is that the reliability of the achievement measures increases with grade, which explains the generally declining residual variance of the four cohorts' later achievements. In this explanation, the relatively lower reliability of the assessment at the start of the first grade is the only reason that later achievement depends substantially less on earlier achievement for Cohort 1. A second possible explanation involves the measurement characteristics of the achievement tests themselves. Tests at the higher levels contain items more variable in terms of grade-level appropriateness, so that at the early grades all the items maybe ~~are~~ very nearly appropriate for students, but at higher grades many items might be ~~are~~ too easy or too difficult. If this were the case, then schooling at the higher grades might ~~cannot~~ be not as directly relevant to ~~as~~ STET much of the test as it could ~~can~~ be at the earlier grades (e.g., a compensatory class in grade six focuses on low-level skills and may be very successful at it, but it does not focus much on high-level skills; therefore even a successful compensatory program would ~~will~~ affect less of the test variance at higher grades than it would be at lower ones.)

We do not believe these alternative explanations are as satisfactory as the substantive interpretaion. Certainly, the evidence is that the CTBS becomes more reliable at the higher grades and that there is a greater difference between reliabilities at grades 1 and 2 than at other adjacent grades. (See the pattern of reliability measures based on internal consistency reported in Hemenway et al., 1978: 42, 44)). For various reasons, related to test-taking inexperience at the start of schooling and the difficulty of administering tests for this first assessment, the reliability of the test is lowest at that time. However, it is doubtful that the particularly low direct effect of initial achievement on later achievement for cohort 1 is due entirely to considerations of reliability. If initial achievement were measured with substantially greater error for cohort 1, we would expect such achievement to affect school experiences with substantially less power as well. In fact, the nonstandardized direct effects of initial achievement on school experiences are ~~the~~ highest for cohort 1 (.19) and fairly constant (.14 to .15) for the other three cohorts. (See Table C-3 in Appendix C. The standard error for these estimates is about .01 for each cohort.)

Two statistical factors combine to create the smaller standardized effect of earlier on later achievement for cohort 1 ~~than for~~ compared to the other three cohorts. This effect follows from the fact that the standardized structural coefficient is a product of the metric coefficient and the ratio of the standard deviation of the predetermined variable of interest to that of the current endogenous variable. First, the metric direct effect of initial achievement on later achievement is lowest for cohort 1 (.76, compared with .83, .88, and .86 for the remaining three cohorts in order; see Table C-3). Second, and much more important,[6] as evident from Table C-2 the ratio of the standard deviations of initial achievement to that of later achievement is lowest for cohort 1 (.58, .78, .74, .76 for the four cohorts in order). In particular, children enter the first grade with more closely grouped abilities than occurs at any later point in their educational careers. As they proceed through school, some children acquire more skills than others, so that the range and standard deviations of scores increase with grade level. (The minima and maxima of total achievement scores in fall 1976 for the six cohorts of the cross-section sample are in order of cohort [207, 411], [225, 566], [269, 641], [284, 679], [332, 781], and [349, 791].) If we consider any three-year period of elementary school, the first three years show the most dramatic expansion of a cohort's achievement levels. Something happens within these years to create greater differences than existed before and a relatively greater increase in variation than within any other three-year period. Aside from probable maturational variations among students, our results indicate that a significant factor is the educational experiences they encounter, represented by the direct effect of X on A .

Equality of Educational Opportunity

We have found that school experiences depend primarily on initial achievement and school quality together rather than on socioeconomic background, but that background is a significant determinant even after controlling for prior abilities and school. We have also found that schooling has a substantial impact on achievement in the earlier years, but that its effects diminish to extremely modest levels by the end of elementary school. Thus, while school services continue to depend on background, school, and initial achievement in a fairly uniform manner across the four cohorts observed, the effects of such services and of the school itself on achievement decline over the student's educational career.

Finally, we have found that despite the palpable effects of schooling for every cohort, particularly the first, the association between background and achievement, already strongly established at entry into school, remains more or less constant throughout the six years of elementary schooling. We recall the EEO report's declaration, on the basis of cross-sectional data, that the "inequalities imposed on children by their home, neighborhood, and peer environment are carried along to become the inequalities with which they confront adult life at the end of school" (Coleman et al., 1966: 325). The present, longitudinal data, although covering only the first half of the 12 grades of schooling, are strikingly consistent with this declaration.

These results raise two important and intimately related questions. First, on a more technical note, how can we reconcile the appreciable effects of schooling reported in this and the previous chapter with the constancy of the background-achievement relationship in evidence for every cohort, including the first? Second, on a broader plane, what are the implications of these results for the extent to which schools provide educational opportunities? We shall address these questions in reverse order.

Schooling and Educational Opportunities. According to the EEO report, "equality of educational opportunity through the schools must imply a strong effect of schools that is independent of the child's immediate social environment, and that strong independent effect is not present in American schools" (Coleman et al., 1966: 325). As noted in Chapter 1, it is still unclear whether the lack of a strong independent effect, if any, is due to a modest effect of schooling on the development of academic skills, to the favorable distribution of educational resources to children of richer social backgrounds, to the strong effects of highly correlated background and ability factors, or to some combination of all ~~of~~ these processes. The present findings indicate how these factors combine to affect educational opportunities during elementary schooling. Again, we find the basic model useful for formalizing the problem.

What the model can tell us about equality of educational opportunity depends on one's definition of that concept. Before considering the alternatives, it may be helpful to consider the meaning of equality of (socioeconomic achievement) opportunities *after* schooling, on which there may be greater agreement. In that context, we may accept the notion that "[c]omplete equality of opportunity exists when the social and economic status a person has is determined by his own abilities and efforts rather than by the circumstances of his birth" (U.S. Department of Health, Education, and Welfare, 1969: 15), even if there may be disagreement about how much we want to base our society's rewards on talent or, more precisely, certain talents. In the context of education, however, beyond the notion that the quality of schooling should not be based on one's social origins, there may or may not also be the notion that schooling--in particular, the development of cognitive skills--should be based on intellectual ability and motivation. On the one hand, the principle that a child should be given educational resources to enable him to go as far as his abilities and motivation can take him is probably acceptable if not cherished. On the other hand, if school resources are allocated on the basis of fallible measures of intellectual ability (IQ or other test scores) ~~that~~ measuring initial skill levels that are highly dependent on social background, then such an allocation criterion can serve "as a kind of cement which fixes students into the social classes of their birth" (Sexton, 1961: 51). Under the circumstances just described, allocation of school resources on the basis of initial achievement would reinforce advantages resulting from the environment into which one is born, while a uniform distribution of such resources would increase the relative achievement of children who suffer socioeconomic disadvantages through accident of birth.

In the previous section, we examined the direct effects of background, schools, and initial achievement on educational services and concluded that such services are distributed primarily on the basis of the latter two factors combined. We also noted, in terms of the variance explained in school services, that even if the indirect influence of background through its association with school quality and initial achievement were taken into account, there is a large component of variance unexplained by background.

The first of these two findings is sufficient to characterize educational opportunities if one is merely concerned with whether or not ability is the primary determinant of school services. The second is important if one accepts (as we and the authors of the EEO report have) the notion that equality of opportunity means that school resources must be allocated and skills developed on a basis independent of background and related characteristics, not merely on some basis--whether initial ability or any other criterion--that by its nature functions in part as a surrogate for socioeconomic status. Therefore, in the remainder of this section, we will weigh the direct and indirect influences of background against the independent effects of initial achievement and school factors by 'breaking apart' the effects of the nonbackground factors and allocating those portions that are due to the covariance of these factors with background to the influence of background. The remaining portions of the effects of initial achievement and school factors will be the effects of these factors 'independent of background'. Such effects will then be compared with the 'augmented' background effect, which represents, in a sense, the direct and indirect influences of background.

Consider, first, the distribution of school services (X). From the first member in Eq. (3.2),

$$
\begin{aligned}
X &= \pi_{XA_O}A_O + \pi_{XS}S + \pi_{XB}B + \varepsilon_X \\
&= \pi_{XA_O}(A_O - \rho_{A_OB}B + \rho_{A_OB}B) + \pi_{XS}(S - \rho_{SB}B + \rho_{SB}B) \\
&\quad + \pi_{XB}B + \varepsilon_X \\
&= \pi_{XA_O}(A_O - \rho_{A_OB}B) + \pi_{XS}(S - \rho_{SB}B) + (\pi_{XB} + \pi_{XA_O}\rho_{A_OB} \\
&\quad + \pi_{XS}\rho_{SB})B + \varepsilon_X,
\end{aligned}
$$

where ρ_{A_OB} is the correlation between A_O and B, and ρ_{SB} the correlation between S and B. Thus,

$$X = \pi^*_{XA_O}A^*_O + \pi^*_{XS}S^* + \pi^*_{XB}B + \varepsilon_X, \tag{3.3}$$

where

$$\pi^*_{XA_O} = \pi_{XA_O}\sqrt{1 - \rho^2_{A_OB}}$$

$$\pi^*_{XS} = \pi_{XS}\sqrt{1 - \rho^2_{SB}}$$

$$A^*_O = (A_O - \rho_{A_OB}B)/\sqrt{1 - \rho^2_{A_OB}}$$

$$S^* = (S - \rho_{SB}B)/\sqrt{1 - \rho^2_{SB}}$$

and

$$\pi^*_{XB} = \pi_{XB} + \pi_{XA_O}\rho_{A_OB} + \pi_{XS}\rho_{SB}.$$

Note that $\mathrm{var}A^*_O = \mathrm{var}S^* = \mathrm{var}B = 1$, so that $\pi^*_{XA_O}$, π^*_{XS}, and π^*_{XB} are the standardized structural coefficients for, respectively, the portion of A_O independent of B, the portion of S independent of B, and the sum of B and the portions of A_O and S associated (i.e., perfectly correlated) with B.

In the earlier sociological literature on path analysis (e.g., Duncan, 1966; Land, 1969), the parameter π^*_{XB} was called the total effect of B on X and was a parameter of central interest. More recently, the term 'total effect' has been used in the sense adopted herein (see Alwin and Hauser, 1975) and π^*_{XB} is now called the 'total association', ~~since~~ because $\pi^*_{XB} = \rho_{XB}$. (The equation for π^*_{XB} above therefore represents a decomposition of the correlation or 'association' between X and B.) We shall instead refer to π^*_{XB} as the 'augmented' effect of background on schooling.

The justification for considering π^*_{XB} as an effect is as follows. π^*_{XB} is the total effect of B on X if one posits a somewhat different model than in Eq. (3.2), namely, one in which the correlations between initial achievement and background and between school quality and background arise from a *causal dependence* of initial achievement and school quality on background. This assumption may be contrary to fact, because ~~since~~ as noted earlier, some of the association between the home environment and achievement is due to the effects of achievement on parental behavior. Thus, the augmented background effect may tend to overrepresent the influence of social origins by attributing all common influences of background with the other exogenous factors to itself. This tack is useful, however, if we suspect that background is an important determinant of initial abilities and the quality of the school attended, in that we obtain the minimal independent effects of schooling and ability, assuming that the B composite captures most of the background characteristics of the student. We shall refer to π^*_{XS} and $\pi^*_{XA_O}$ as the 'independent' effects of S and A_O (effects 'independent of background').

Table 3-2 reports the maximum-likelihood estimates of the coefficients of Eq. (3.3) and some other results as well. The coefficients in order of row for the first panel are:

(1) the direct effect of background (π_{XB})

(2) the augmented effect of background (π^*_{XB})

(3) the independent effect of school quality (π^*_{XS})

(4) the independent effect of earlier achievement ($\pi^*_{XA_O}$)

(5) the combined effect of school and initial achievement, independent of background, namely, $(\pi^{*2}_{XA_O} + \pi^{*2}_{XS} + 2\pi^*_{XA_O}\pi^*_{XS}\rho_{A_O^*S^*})^{(1/2)}$, which corresponds to Heise's (1972) 'sheaf coefficient.' In essence, the effect is of the linear combination $\pi^*_{XA_O}A^*_O + \pi^*_{XS}S^*$ after standardization (i.e., division by the standard deviation of the linear combination). Substantively, the coefficient is the combined effect of those portions of initial achievement and school quality that are independent of background. ~~and~~

(6) the effect of ε_X when standardized.

The second panel of the table reports results when S is considered ~~as~~ another indicator of background. As noted above,

Table 3-2

Effects of Background, of Portions of School Factors and Initial Achievement Correlated With Background, and of the Portions of These Factors Independent of Background on School Experiences

Background effects: school and achievement effects independent of background

Effect	Cohort 1	2	3	4
1. Direct effect of student background	.280	.273	.287	.278
2. Augmented effect of student background	.539	.528	.609	.531
3. Independent effect of school characteristics	.177	.217	.200	.073
4. Independent effect of total achievement, fall year 1	.353	.361	.358	.417
5. Independent combined effect of school characteristics and total achievement, fall year 1	.421	.445	.425	.423
6. Effect of standardized disturbance	.730	.724	.670	.734

Background and school effects: achievement effects independent of background and school

Effect	Cohort 1	2	3	4
1. Direct combined effect of student background and school characteristics	.372	.392	.408	.303
2. Augmented combined effect of student background and school characteristics	.592	.598	.664	.540
3. Effect of total achievement, fall year 1, independent of student background and school characteristics	.348	.358	.357	.417
4. Effect of standardized disturbance	.730	.724	.670	.734

school characteristics may function as a proxy for neighborhood factors rather than as an indicator of school quality. Hence, the coefficients in order of row are:

(1) the combined effect of B and S on X;

(2) the augmented effect of these two variables when their association with A_0 is considered to reflect influences of B and S on A_0; and

(3) the effect of A_0 on X independent of B and S.

As the first panel of Table 3-2 shows, there is a substantial difference between the direct effect of a student's background on his educational experiences and the augmented effect of background, when we consider the latter's association with school quality and initial achievement. For cohort 1, which is typical of all the cohorts, the direct effect is about one-half of the augmented effect. Thus, because of the substantial correlation between, on the one hand, the child's socioeconomic background and, on the other, his academic abilities and school's quality at the start of his educational career, the augmented effect of background on schooling can be much greater than the direct effect of background alone. Clearly, those who are privileged by background are likely to be given more educational resources, even if much of this is due directly to initial academic abilities and choice of school and only indirectly to background. Fully half of the association between background and school experiences is due to the preferential treatment that socioeconomically privileged children receive, while the other half is due to the association of background with ability and school quality.

Table 3-2 (first panel) also shows that the independent effects of the school and of prior academic abilities are substantial. Their combined independent effect is not as large as the augmented background effect, but when one also considers the effect of other factors uncorrelated with background, school, and initial abilities (represented by ε_X), the effects independent of background are much greater than the augmented background effect (e.g., for cohort 1, $\sqrt{.421^2 + .730^2} = .842 > .539$). The effect of ε_X (when standardized) represents factors that were earlier characterized as 'randomizing' the assignment of school resources to some degree. These factors in combination with assignment on the basis of school quality and initial achievement, which remain largely independent of background, provide important opportunities independent of background for obtaining school resources.

These results are essentially duplicated even when we consider school quality as simply a surrogate for neighborhood characteristics (Table 3-2, second panel). The direct and augmented effects of background are increased and the independent effect of initial achievement reduced in relation to the independent effect of initial achievement and school quality combined, but the relative strength of background and correlated factors on the one hand and independent factors on the other remain essentially the same.

Educational opportunity must be assessed not only in terms of the dependence of school experiences on background and independent factors, but also, in terms of the resulting levels of academic skills achieved. The calculation of augmented and independent effects now becomes somewhat more complicated because of the fact that there is an intervening variable (X) between the exogenous factors and later achievement. While there is no unique way of proceeding, we shall find the following effects useful for characterizing the schooling process:

(1) the direct effect of background (π_{A_3B})

(2) its indirect effect through X ($\pi_{A_3X}\pi_{XB}$)

(3) the augmented direct effect of background ($\pi_{A_3B} + \pi_{A_3S}\rho_{SB} + \pi_{A_3A_O}\rho_{A_OB}$)

(4) the augmented indirect effect of background through X $(\pi_{A_3X}\pi^*_{XB})$;

(5) the independent direct effect of earlier achievement (say, $\pi^*_{A_3A_O}$);

(6) the independent indirect effect of earlier achievement through X $(\pi_{A_3X}\pi^*_{XA_O})$;

(7) the independent direct and indirect effect of school quality $(\pi^*_{A_3S} + \pi_{A_3X}\pi^*_{XS})$; and

(8) the effect of X independent of all prior (exogenous) factors $(\pi_{A_3X}\sqrt{var\varepsilon_X})$.

These eight effects are represented in Table 3-3, along with $\sqrt{var\varepsilon_{A_3}}$, the effect of ε_{A_3} when standardized. We have not shown the results when S is considered as another background factor, as ~~since~~ these results are practically identical with those reported in the table.

Table 3-3

Effects of Background, of Portions of School Factors and Initial Achievement Correlated With Background, and of the Portions of These Factors Independent of Background on Later Achievement ~~on Later Achievement~~

	Effect	Cohort			
		1	2	3	4
1.	Direct effect of student background	.076	.135	.110	.136
2.	Indirect effect of student background (thru three-year total school experiences)	.099	.017	.050	.019
3.	Augmented direct effect of student background	.302	.448	.486	.495
4.	Augmented indirect effect of student background	.189	.032	.107	.034
5.	Independent direct effect of total achievement, fall year 1	.378	.563	.529	.629
6.	Independent indirect effect of total achievement, fall year 1	.124	.022	.063	.027
7.	Independent direct and indirect effects of school characteristics	.062	.095	.035	.025
8.	Effect of three-year total school experiences independent of all prior variables	.257	.044	.118	.046
9.	Effect of standardized disturbance	.652	.619	.528	.536

The first two rows of Table 3-3 display the direct and indirect effects of background on later achievement and are analogous to the first row of Table 3-2. When the association of background with other factors is not taken into account, the net effect of background is clearly modest and, on average over the four cohorts, does not dominate the direct effects of schooling. Rows 3 and 4 of the table show, however, that once the association of background with initial achievement and school quality are taken into account, the influence of background on later achievement is substantially augmented. Since school quality has no direct effect on later achievement for most of the cohorts, the augmented direct effect of background is mostly due to its association with initial achievement and the large effect of the latter on later achievement. In addition, for two of the four cohorts that exhibit substantial effects of school experiences on later achievement, the augmented indirect effect of background, which is the result of the direct effect of school experiences on achievement and of the association between such experiences and background, is substantial.

Row 5 of the table presents the direct effect of earlier achievement, independent of background, on later achievement. Comparison of the figures in this column with the direct effects of earlier on later achievement in Table 3-1, shows that the bulk of the effect of prior ability on achievement is unrelated to background factors.

Rows 6 to 8 of Table 3-3 represent the 'expected' effects (in a normative rather than statistical sense) of the schooling process on achievement. The first effect is that of prior ability, independent of background and mediated by the services received. This effect cannot be attributed entirely to schooling, since

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initial ability is involved, but the effect does represent an effect of the schooling process as it is expected to operate insofar as resources are allocated on the basis of ability and to facilitate further achievements. The second effect is that of school quality, independent of background, both directly and through the educational services received. Since for most cohorts, there is no direct effect of S on A_3, this effect is predominantly indirect. The third effect is that of school services independent of background, school quality, and initial achievement.

Finally, row 9 represents the effect of residual factors uncorrelated with all prior, explicit factors in the model (X, A_0, S, and B). It indicates that much of the variation in later achievement is unexplained by background, schooling, or prior abilities.

The pattern of results confirms our earlier conclusions that schooling influences are substantial soon after the child begins his education, but that these influences decline as the child advances through school. In addition, if we compare the influences of schooling independent of background with the direct and indirect influences of background, there is little doubt that for all but the first cohort, background is the stronger influence.

Somewhat counterbalancing the strong direct and indirect influences of background in the schooling-achievement process, is not so much the influence of schools, but rather the influence of

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sheer ability, independent of background. In other words, opportunities for advancement of one's academic skills independent of background are, though modestly provided by schools, substantially enhanced by the fact that ability alone is a large determinant of academic success.

In short, while schools provide important opportunities for receiving educational resources independent of background, the effects of those resources independent of background are relatively small in general and certainly insufficient to alter the background-achievement relationship.

Finally, we have observed that while factors independent of social background affect schooling and achievement, the relationship between background and achievement established prior to entry into school is undiminished by the educational process. The present results indicate that this outcome is the result of all three of the following: a strong effect of highly correlated background and ability factors, the favorable distribution of educational resources to children of privileged backgrounds--only in part due to preferential treatment on the basis of background, and the generally modest effects of schooling on achievement.

The Stability of the Background-Achievement Relationship. We have seen that prior achievement is the primary determinant of later achievement for all the cohorts (Table 3-1). When this influence is combined with the high correlation between background and initial achievement, a firm base is provided for the preservation of the background-achievement relationship. When to this base is added the direct effect of background, we obtain most if not all of its augmented direct effect (row 3 of Table 3-3), since the component due to the direct effect of school quality and its association with background is nil in most cases and modest in the exceptional case (cohort 2).

The 'fitted' correlation between background and later achievement--that is, the correlation implied by the (generally unsaturated) model (for each cohort) in Figure 3-2 and the estimates of the structural parameters under the model--is the sum of rows 3 and 4 of Table 3-3. Hence, for cohorts 2 and 4, the strong effect of highly correlated background and prior ability factors alone accounts for most of the association between background and later achievement. Moreover, row 4 (the augmented indirect effect of background) shows that the effects of school experiences on achievement were so modest that despite the high correlation between social origins and school experiences ($\rho_{XB} = \pi^*$; row 2 of Table 3-2), the children in these cohorts were unable to capitalize much on their better schooling to enhance their relative academic standing significantly.

On the other hand, in the case of cohorts 1 and 3, the contribution to the background-achievement correlation of the effect of school experiences and its association with origins is appreciable. For these cohorts, however, when school influences were large enough to make some difference, the high correlation between background and school experiences only enhanced the relative academic standing of children of privileged backgrounds.

If we were to eliminate the partial association between social origins and school quality given prior achievement and remove the direct effect of origins on school experiences (while keeping the other structural parameters constant), we would remove all partial association between background and schooling given prior achievement. This partial association may be considered to be the result of 'discriminatory' treatment that children of disadvantaged backgrounds (of the same initial achievement as that of their privileged grademates) receive. While such treatment accounts for some of the perpetuation of the background-achievement relationship, elimination of such treatment would have a small impact beyond the earliest years of schooling (represented by cohort 1). ~~This is so because~~ the preferential treatment that socioeconomically privileged children received over the treatment that they would have received solely because of their prior abilities and schools accounts for at most about one-half of the augmented indirect effect of background. For the four cohorts in order, the contributions to the correlations between background and later achievement from the remaining correlations between background and schooling (S, X), after removing the preferential treatment of socioeconomically privileged students, are .108, .029, .056, and .018 (cf. row 4 of Table 3-3). In order to remove these contributions, which are small taken alone except in the case of cohort 1, the association between background and schooling would have to be made nil by 'discriminating' against privileged children, that is, by making negative both the partial association between origins and school given initial achievement and the direct effect of background on school experiences ~~negative~~ 0. For the four cohorts in order, the partial correlations between background and school quality given prior achievement would have to be -.170, -.129, -.183, and -.056, while the standardized direct effects of background on school experiences would have to be -.211, -.201, -.254, and -.238, in order for the correlations between background and both of the schooling variables to be zero. In order further to reduce the relationship between background and schooling and thereby reduce the correlation between background and later achievement, the preferential treatment accorded to the disadvantaged would have to be additionally increased or else we would have to devise alternative and substantially compensatory educational programs for them, something we have not yet been able to accomplish.

The requirement that the partial relations between background and schooling be negative in order to reduce substantially the correlation between background and achievement over time is reminiscent of the suggestion of Jencks et al., (1972: 109) that short of drastically restricting the amount of schooling (in years of exposure) given to academically advantaged children, schools have little potential ability to reduce inequalities in cognitive skills. Thus, when schooling does affect achievement substantially, as in the early years, the background-achievement relationship is preserved in some significant part by the association of background with schooling, only half of which is due to any preferential treatment on the basis of social origins.

A substantial reduction in the background-achievement relationship over time will occur, all other things being equal, only if schooling has a greater impact on achievement (among those attending school) and if school resources are to a lesser degree distributed to children of privileged backgrounds--in short, only if schooling has a greater effect on achievement independent of background. This can be seen from the decomposition of the correlation between background and later achievement:

$$(3.4) \qquad \rho_{A_3B} = \pi_{A_3X}\rho_{XB} + \pi_{A_3S}\rho_{SB} + \pi_{A_3A_O}\rho_{A_OB} + \pi_{A_3B}.$$

If we increase the metric coefficients corresponding to π_{A_3X} and π_{A_3S}, we create greater variation in A_3, which while not large enough to offset the increases in π_{A_3X} and π_{A_3S} following from the increased metric coefficients, would reduce $\pi_{A_3A_O}$ and π_{A_3B}. If at the same time, we reduce ρ_{XB} and ρ_{SB} by allocating school resources solely on the basis of ability, we could achieve a reduction in all terms on the right-hand side of Eq. (3.4) or at least allow the reduction in $\pi_{A_3A_O}\rho_{A_OB} + \pi_{A_3B}$ to offset any increase in $\pi_{A_3X}\rho_{XB} + \pi_{A_3S}\rho_{SB}$ due to the increases in the effects π_{A_3X} and π_{A_3S}.

Appealing as this argument is, however, it is surprising how little impact we would have on the association between background and achievement with a substantial increase in schooling effects and the elimination of preferential treatment of socioeconomically privileged students. As an illustration of this point, consider cohort 3 and suppose we could increase the metric direct effect of X on A_3 from the observed value (.691) to the value for cohort 1 (1.309; Table C-3 of Appendix C). Suppose also that all the other (estimated) structural coefficients remain constant, except for the (estimated) variance of A_3, which will increase as a result of the greater effect of X (assuming the residual variance of A_3 remains constant). As a result of these changes, we increase p_{A_3X} from .176 to .300, a size comparable to the effect for cohort 1 and involving a multiplicative factor of 1.705.

Because p_{A_3B} and $p_{A_3A_0}$ are reduced somewhat by the increase in the variance of A_3 (these coefficients are being reduced by a factor of .899), the augmented direct effect of B declines from .486 to .437. On the other hand, the increase in p_{A_3X} causes the augmented indirect effect of B to increase from .107 to .183, making the correlation between B and A_3 .620 instead of the 'fitted' value of .593. Thus, the increase in the schooling effect has thus far augmented the background-achievement relationship. If we now eliminate the partial correlation between S and B given A_0 and the direct effect of B on X, we reduce the augmented indirect effect of B by .097 and the correlation between B and A_3 likewise to .522, a decline of only .071 from the 'fitted' value. If this decline seems small, recall that simply eliminating the partial relationship between background and schooling for cohort 3 without increasing p_{A_3X} would have reduced the correlation between B and A by .051. Thus, the sizeable increase in the schooling effect (combined with the elimination of the partial relationship between background and schooling given prior achievement) has created virtually no difference.

In short, ~~we have the~~ perpetuation of the status-achievement relationship is well established before entry into school, not primarily because schools discriminate in their allocation of resources, nor because the direct and indirect effects of background in themselves are overwhelming, but rather because these effects are combined with generally modest schooling influences that even when otherwise are nevertheless substantially linked to the child's background both directly and indirectly through his prior achievement. In turn, his social background is substantially related to his prior achievement at any given point in his educational career and such achievement is the primary determinant of his later achievements.

Differences Between Reading and Math Achievement

Our evaluation of the schooling process has up to now involved the total achievement score, which combines performance on reading and math tests. To be sure, performance in the two areas is highly correlated (Table C-2, Appendix C; Jencks et al., 1972: 54-5), suggesting a common underlying dimension. Our earlier analyses and those reported in Chapter 4 are intended to evaluate the effects of background and schooling on this common dimension. Nevertheless, previous research indicates that the separate achievement tests are differentially related to other variables. Jencks and Brown (1975) reported that high schools that were effective in terms of one achievement test were not generally effective in terms of others. More significantly, socioeconomic background has been found to exert a stronger influence on reading than on math achievement (Beal et al., 1979; Haertel and Wiley, 1979), an observation consistent with our expectation that math is not as easily learned outside of school as are verbal skills. In addition, school influences may be more pronounced in development of math skills, as has been found to be the case when the effects of desegregation are evaluated (St. John, 1975).

To test these hypotheses, we elaborated the basic model (3.2) by decomposing the total achievement score into the reading and math components. In the new model, achievement in fall 1976 (A_O) is replaced by reading and math achievement in the same period (R_O and M_O, respectively). Similarly, spring 1979 achievement (A_3) is replaced by the separate skill-area scores (R_3, M_3). The equations now become

$$
\begin{aligned}
& X = \pi_{XR_O} R_O + \pi_{XM_O} M_O + \pi_{XS} S + \pi_{XB} B + \varepsilon_X \\
(3.5) \qquad & R_3 = \pi_{R_3X} X + \pi_{R_3R_O} R_O + \pi_{R_3M_O} M_O + \pi_{R_3S} S + \pi_{R_3B} B + \varepsilon_{R_3} \\
& M_3 = \pi_{M_3X} X + \pi_{M_3R_O} R_O + \pi_{M_3M_O} M_O + \pi_{M_3S} S + \pi_{M_3B} B + \varepsilon_{M_3} .
\end{aligned}
$$

Again, there are a few methodological details, some with important substantive aspects, to which we must attend before discussing the results.

We assume that the disturbances for R_3 and M_3, namely ε_{R_3} and ε_{M_3}, respectively, may be correlated, since we do not expect to account perfectly for the correlation between R_3 and M_3 by the predetermined variables. Alternatively, we could posit that R_3 directly affects M_3 or vice versa, but not both, unless we are prepared to assume, for example, that the 'simultaneous' effects are equal or that at least one predetermined variable does not directly affect one of the measures of later achievement. The reason for these restrictions is that without them we would have more structural parameters to estimate than data available (in the form of the covariances among the endogenous variables and between them and the exogenous variables). In the terminology of structural-equation modeling, in this situation the model would be 'underidentified'; in essence, some of the structural parameters would be indeterminate, having an infinite set of possible values consistent with the data. Thus, however the model is conceived, not all possible structural relations can be assumed to be arbitrary, that is, nonzero and unconstrained (except by the requirement that the covariance matrix of the variables in the system is positive definite). In actuality, these kinds of constraints are not new. Within the recursive model (3.2), we assumed that the disturbances were uncorrelated for exactly the same reason; the system was 'just-identified' with this restriction and would have required some other restriction on the structural parameters had we allowed the disturbances to be correlated.

We assume that the disturbances of the later achievement measures are correlated rather than assuming that either affects the other directly or that they simultaneously affect each other with a single additional constraint on these effects or on the other structural parameters because the alternatives are less plausible and in a sense more restrictive. The correlation of the disturbances simply acknowledges that there is a residual correlation between R_3 and M_3 that cannot be accounted for by the predetermined variables and their effects on the achievement measures.

The assumed correlation between the disturbances for R_3 and M_3 is diagramatically represented in Figure 3-3 by the curved, double-headed arrow connecting ε_{R_3} and ε_{M_3}. It is evident from the diagrams for the four cohorts that there is indeed a substantial residual correlation between the achievement measures, so that a model that did not allow for or somehow attempt to account for this correlation would be unrealistic. In the saturated model (3.5), this correlation is simply the partial correlation between R_3 and M_3, given the predetermined variables.

The model (3.5) entertains the possibility of direct effects of prior reading on later math achievement and of prior math on later reading achievement. These effects are depicted in Figure 3-3. Of the two, the first is perhaps more apparently plausible, ~~since~~ because it is likely that reading skills are a prerequisite for just about any other academic skill, including math. On the other hand, the supposition that initial math ability directly affects later reading achievement is less substantively apparent. The results of Figure 3-3 show that within the model (3.5) such effects cannot be ruled out without rendering the reproduced correlations among the variables substantially at odds with the observed pattern of relations among the variables. The models adopted thus retain the second effect, but do not explain it. It should be kept in mind, however, that the direct effect of earlier math ability on later reading achievement in the model need not indicate that such an effect literally occurs. It simply means that the model does not entertain a variety of intervening mechanisms by which earlier math skills can affect

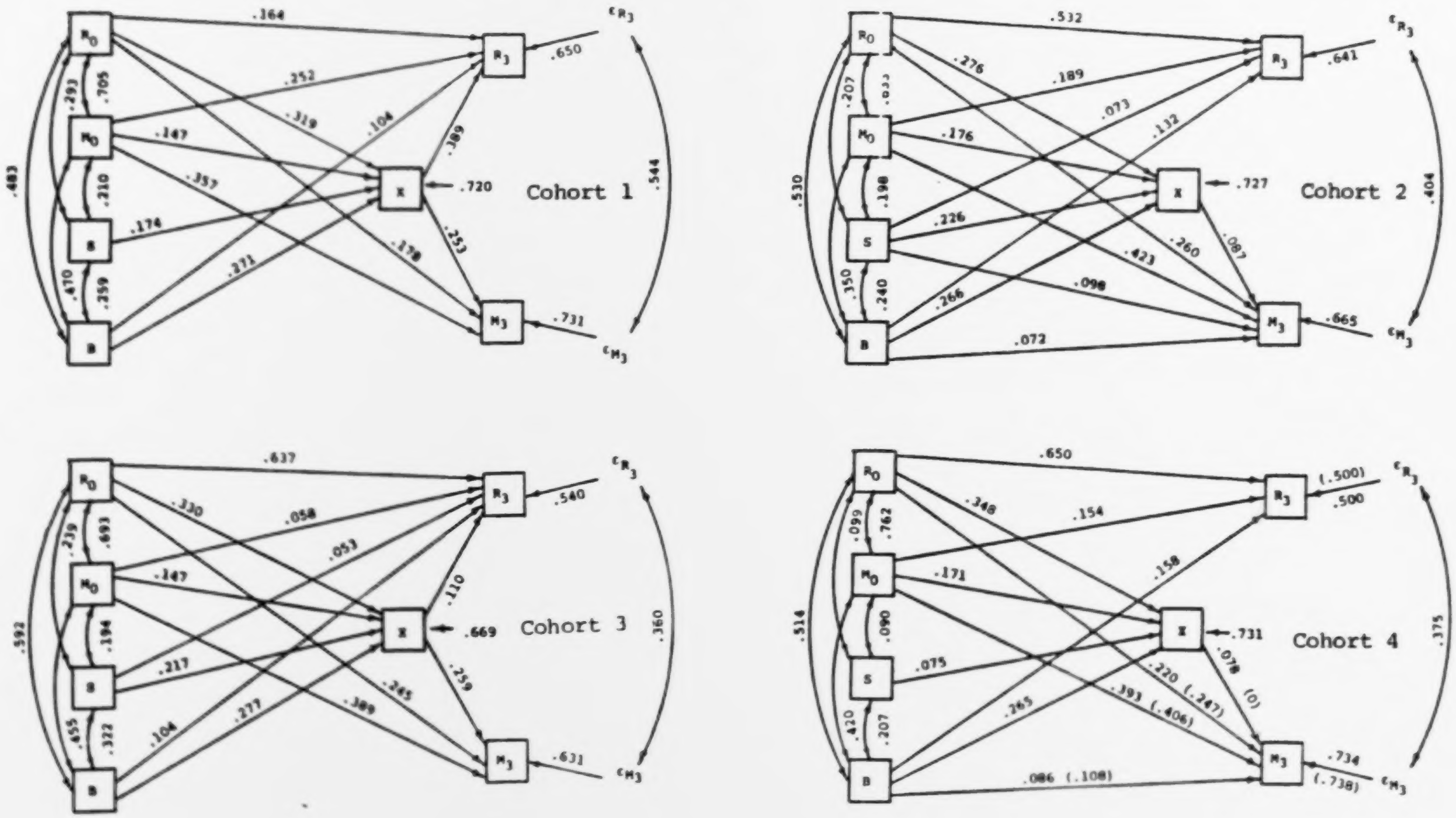
R_0
M_0
S
B
X
R_3
M_3
ϵ_{R_3}
ϵ_{M_3}
.164
.252
.319
.104
.147
.357
.389
.720
Cohort 1
.544
.650
.731
.705
.293
.483
.210
.174
.271
.178
.253
.470
.359
.532
.276
.189
.073
.132
.176
.641
.727
Cohort 2
.404
.207
.530
.198
.226
.260
.423
.087
.098
.266
.350
.240
.072
.665
.637
.330
.058
.053
.147
.110
.669
Cohort 3
.360
.540
.631
.693
.239
.592
.194
.217
.104
.277
.245
.389
.259
.455
.322
.650
.348
.154
.158
.171
(.500)
.500
.731
Cohort 4
.375
.099
.762
.514
.090
.075
.265
.220 (.247)
.393 (.406)
.078
(0)
.420
.207
.086 (.108)
.734
(.738)

Figure 3-3

Path Diagrams With Estimates of Standardized Coefficients for the Model of Equation 3.4, With Nonsignificant Effects Omitted later reading achievement. The model thus represents the total effect between the two simply as a direct effect, when indirect effects may constitute most or all of the total effect. For example, a student who succeeds in math may be more motivated to improve his reading skills as well. Encountering success in one area of academics, he may be encouraged to succeed in others. Naturally, such indirect effects may also partially account for the direct effect of earlier reading ability on later math achievement. Had the model included appropriate intervening mechanisms, the direct effects from reading to math and conversely would undoubtedly have diminished.

While the model treats the student's achievements in reading and math as distinct, it does not distinguish between his school experiences in the two areas. This tack was taken not merely because of any difficulty in allocating some of the school experiences (HR-RACE, WEEKS) between reading and math. These variables could have been considered in the schooling composites for each skill-area along with the remaining indicators, which were available by area. However, we strongly suspected that were separate schooling composites developed, we would nevertheless have found direct effects of each on both areas of achievement because each variable is probably an indicator of school experiences not only in its nominal area, but also in the complementary one as well. Moreover, both composites would probably have contained components in common (namely, HR-RACE and WEEKS). ~~Since~~ As the differential effects of school experiences in reading and math on achievement in any given area ~~was~~ were not of interest, but rather the total effects of schooling as a whole, we continued to use the single school-experience composite (X).

Finally, because of the correlated disturbances, the model (3.5) is no longer recursive. Nevertheless, in the case of the saturated model, with which we began ~~for all cohorts,~~ the maximum-likelihood estimates of the structural coefficients can be (and were) obtained by OLS, and the estimated residual correlation was computed as the partial correlation described above. These values were then employed in a maximum-likelihood routine (LISREL; Jöreskog and Sörbom, 1978). The nonsignificant paths (at the .05 level, except as noted below), as indicated by analysis of the χ^2 statistic for the log-likelihood ratio test, were deleted by a kind of 'backward elimination' procedure. ~~with~~ those paths contributing least to the fit of the model to the observed correlation matrix were eliminated first. The fit of the final, unsaturated models to the data as indicated by the χ^2 statistic for the four cohorts in order are: # (1) $\chi^2_3 = 3.19$, $p = .36$; # (2) $\chi^2_1 = 1.79$, $p = .18$; # (3) $\chi^2_2 = 2.00$, $p = .37$; and # (4) $\chi^2_3 = 2.42$, $p = .49$, where χ^2_p is the χ^2 statistic with p degrees of freedom. # (For cohort 4, the deletion of π_{M_3X} increases χ^2 to $\chi^2_4 = 6.08$, $p = .19$, with $\chi^2_1 = \chi^2_4 - \chi^2_3 = 3.66$, $p = .056$. Thus, P_{M_3X} is barely nonsignificant, given that $\pi_{R_3S} = \pi_{M_3S} = \pi_{R_3X} = 0$. Figure 3-3 shows the two sets of estimates # -- # when π_{M_3X} is unrestricted and when it is assumed to be zero.)

The final models fitted (Figure 3-3), Table 3-4 , indicate a clear distinction in the relations of reading and math achievement to background and school factors. (The metric estimates are

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reported in Table C-4.) The results can be divided into three areas, corresponding to the nature of the predetermined variables (background, schooling, and prior abilities).

Table 3-4

Estimates of Standardized Direct Effects for the Models of Figure 3-3

Endogenous Variable	Predetermined Variable				
	Three-Year Total School Experiences	Reading Achievement Fall Year 1	Math Achievement Fall Year 1	School Characteristics	Student Background
Cohort 1					
Three-year total school experiences	---	.319	.147	.174	.271
Reading achievement, spring year 3	.389	.164	.252	.000	.104
Math achievement, spring year 3	.253	.178	.357	.000	.000
Cohort 2					
Three-year total school experiences	---	.276	.176	.226	.266
Reading achievement, spring year 3	.000	.532	.189	.073	.132
Math achievement, spring year 3	.087	.260	.423	.098	.072
Cohort 3					
Three-year total school experiences	---	.330	.147	.217	.277
Reading achievement, spring year 3	.110	.637	.058	.053	.104
Math achievement, spring year 3	.259	.245	.389	.000	.000
Cohort 4					
Three-year total school experiences	---	.348	.171	.075	.265
Reading achievement, spring year 3	.000	.650	.154	.000	.158
Math achievement, spring year 3	.000	.247	.406	.000	.108
Math achievement, spring year 3*	.078	.220	.393	.000	.086

*Estimates allowing the effect of three-year total school experiences on math achievement, spring year 3, to be nonzero. The estimated effect of three-year total school experiences on math achievement, spring year 3, is barely nonsignificant ($p = .056$).

Note. — The estimate of an effect is zero (indicated as .000) when the estimate under the saturated model was not significantly different from zero. In that case, the unsaturated model was adopted, with the corresponding parameter assumed to be zero.

The dashes (---) indicate that the parameter is not defined under the saturated model.

Background. At the entrance of children into school, the correlations between their backgrounds and their abilities in the two areas of achievement are essentially the same (the correlations are .48 for reading and .47 for math for cohort 1 at the start of the first grade). However, soon after they are exposed to some schooling, the association between background and reading achievement becomes appreciably stronger relative to that between background and math achievement. This pattern is evident in the correlations between the two areas of achievement at the start of the study for the four cohorts (Figure 3-3); for all but the first cohort, there is an appreciable difference between the reading and math correlations with background. Furthermore, as the first cohort proceeds through school, the correlations of reading and math achievement with background become increasingly divergent (Table C-2, Appendix C). Again, reliability considerations do not threaten these conclusions, because, if anything, the evidence cited earlier suggests that the math test is least reliable in the first grade and that the difference in reliabilities of the tests in the two areas is greatest in that grade. Finally, for all the cohorts, there is a stronger (direct) effect of background on reading than on math achievement (Figure 3-3; Table 3-4). In fact, for two of the cohorts, there is no statistically significant effect of background on math achievement.

These results are consistent with earlier work with the SES data (Beal et al., 1978; Haertel and Wiley, 1979), which did not employ the path-analytic framework adopted here. The results are also consistent with our expectation that the home environment more readily encourages and aids ~~the~~ acquisition of verbal rather than ~~of~~ math skills.

Earlier Achievement. A second pronounced difference in the behavior of the reading and math measures is that while in the earliest period of schooling, the (standardized, direct) effect of earlier on later reading achievement is substantially less than the effects for later periods, there is less variation in the effects of earlier on later math achievement. In addition, there is a monotonic increase in the effects for reading that is not evident for math. These results indicate that the pattern of increasing direct effects of earlier on later achievement which we observed earlier for total achievement, is due mostly, if not entirely, to the existence of the same pattern in the area of reading skills. Thus, the determination of later achievement by prior abilities is weakest in the earliest period of elementary school and occurs primarily in reading. As the child progresses through school, his reading skills at any given grade [illegible] depend increasingly on the skills he possessed ~~at~~ some time before.

Still another difference between ~~[illegible]~~ stet reading and ~~reading~~ stet math is that, aside from the relatively low effect of earlier on later reading achievement for the first cohort, later achievement is generally less directly dependent on prior abilities in math than in reading. In other words, one's prior abilities are less important as a determining factor ~~in~~ of one's later achievements in math than in reading.

It is also of interest that all the predetermined variables account for less of the variation in math (M_3) than in reading skills (R_3) (Figure 3-3). This difference appears to arise ~~be the case~~ because math achievement is less dependent on one's social origins and prior abilities, ~~since~~ del as we shall presently observe the effects of schooling are greater for math than for reading.

Schooling. Beyond a brief period following entry into school, school experiences have a greater impact on the development of math skills than they do on the development of reading skills.

For the older three cohorts, the effects of schooling on reading are uniformly less than the effects on math. (For cohort 4, there is some evidence of a greater effect of schooling on math achievement, as indicated by the barely nonsignificant effect of X on M_3.)

Finally, we note that the influence of school factors on achievement appears to decline, as the student progresses through school ~~with respect to~~ in both skill areas. This decline is sharper for reading than for math. As just noted, there is no reliable evidence of schooling effects on reading for cohort 4, but some evidence for schooling effects on math, which probably accounts for the effect of schooling on total achievement, found earlier, for cohort 4.

The patterns of effects of background, schooling, and earlier achievement on later achievement in reading and math are convergent and consistent with our expectations. The home environment is a more influential factor in the development of reading skills than in the development of math skills. Just the opposite occurs, however, with respect to the influence of schooling ~~within~~ for a short time immediately after entry into school. Moreover, as the child progresses through school, the effects of schooling diminish ~~substantially~~ essentially to the zero point in the area of reading, while there is some evidence for continuing, if also reduced, effects in math. And, finally, achievement in math is less determined by prior abilities and social origins than is ~~the case for~~ reading achievement.

All of this evidence suggests that math skills are more responsive to outside intervention than are reading skills. ~~Although~~ the latter may be more important because of their centrality in the development of academic skills in general. (for the older three cohorts, the direct effect of earlier reading on later math achievement is greater than the effect of earlier math on later reading achievement). However, it appears that schools have a better chance of success in developing math skills.

Conclusions

To recapitulate briefly the findings of this chapter ~~in brief~~, educational resources are distributed among students primarily on the basis of their academic abilities and the schools that they attend, as opposed to their social origins. Nevertheless, there is evidence of significant preferential treatment accorded to children of privileged backgrounds above that which they would receive if the schools they attended were the result solely of their abilities and if the resources they received were dependent solely on their schools and abilities. This preferential treatment and the strong association between background and abilities before schooling starts, results in a close association between schooling and background, with each component accounting for about half ~~of~~ the relationship.

While schools exert an appreciable influence on achievement, and their average effects over the elementary years are comparable to the effects of background alone, these effects decline rapidly to modest levels by the end of the elementary period. In the area of reading, the decline is particularly drastic, with no evidence of schooling effects in the later years.

Despite some effects of schooling on achievement, the correlation between background and achievement remains essentially constant throughout the child's elementary schooling, although the background-achievement bond is significantly stronger in reading than in math. This perpetuation of the background-achievement relationship is the result of the strong effect of highly correlated background and ability factors, the favorable distribution of school resources to children of privileged backgrounds, and the generally modest effects of schooling on achievement. Even if the preferential treatment accorded to socioeconomically privileged students were completely eliminated, the impact on the background-achievement relationship would ~~on~~ the whole, be small because of the modest effects of schools. When those effects are substantial, as in the earliest years, the strong association between background and ability prior to school, works to sustain the background-achievement relationship over those years. Our simulations (numerical 'experiments') suggest that even if schools were successful in lengthening the period over which educational experiences had a substantial impact on academic skills, this success would probably only augment the background-achievement relationship unless *major* changes were made in the way educational resources are currently distributed, to the point of according preferential treatment to those of disadvantaged backgrounds.

When our basic model of the schooling-achievement process is elaborated by employing yearly data on school experiences and academic achievement, the results of the simpler model on the relative influences of background, schooling, and earlier achievement are confirmed. In addition, the elaborated model clarifies the direct and indirect effects of earlier factors on later achievement, showing, first, that the receipt of educational resources depends to a greater extent on earlier levels of resources received than on earlier achievement. ~~and,~~ second, it shows that the influence of earlier on later achievement is predominantly direct, with an almost negligible indirect effect through ~~the~~ schooling received. These two results suggest that administrative decisions on the allocation of school resources, once made, are resistant to change on the basis of later achievement, but that the stability of achievement is hardly due to the inertia of the resource-allocation process.

The basic model of Chapter 3 provides a simple basis for evaluating the schooling-achievement process, especially in examining the relative influences of students' social origins, prior abilities, and schooling on their academic achievements. In this chapter, we extend the model by taking advantage of ~~the~~ multi-occasion measurements of school experiences and academic achievement.

The elaborated model provides a means for confirming the characterization of the students' educational development based on earlier inter-cohort comparisons by examining more closely the changes within cohorts.

In addition, the model enables us to explore the role of feedback in the educational process and, in particular, the long-term consequences of earlier schooling and achievement experiences on later achievement. We have already observed that these long-term consequences, linked as they are so strongly to background, result in a perpetuation of the relative achievement differences among children who start out with background-related differences in academic skills. A question of interest concerns the processes accounting for this trend--whether the background-achievement relationship is sustained in part because of the mediated impact of earlier experiences on later ones through a chain of schooling and achievement experiences or because the total impact of earlier experiences is also partly due to continuing, direct effects of these on all subsequent achievement statuses. Either or both of these situations are possible. In the first case, a child's initial disadvantage leads to less favorable schooling experiences that reinforce his achievement deficit, resulting in further deficits in schooling and so on. A chain of events links his academic origins to his achievement at some later point. In the second case, the child's academic origins impinge directly on his achievement at all stages throughout his educational career and the cumulation or sum of these multiple impacts and their long-term, mediated consequences is such as to sustain his achievement deficit over time.

A second question of interest is whether the home and community environment is particularly critical in the earlier years. It may be that the direct effects of background on achievement are strong primarily in the early years, where basic attitudes, values, habits, and skills are developed, and that as these effects diminish in later years, the influence of background on achievement becomes primarily indirect. The results in Chapter 3 suggest that social background exerts a continuing, direct influence on achievement throughout a student's career. With year-to-year data, this appears less likely, as we shall see. Finally, we will be interested in the year-to-year pattern of decline in schooling effects as ~~that were~~ indicated somewhat obliquely by the three-year cohort patterns reported in Chapter 3.

The Extended Model: Examination of the Schooling Process by Year

The extended model is a system of six structural equations, one for each of the annual school-experience and achievement assessments in the three years of the SES. These equations are a straightforward extension of model (3.2). Let school experiences during the ith school year be represented by X_i and achievement at the end of the ith year by A_i, i = 1, 2, 3. The six equations are represented simply in matrix form:

$$\underline{\Pi}_{yy}\underline{y} = \underline{\Pi}_{yx}\underline{x} + \underline{\varepsilon} \qquad (4.1)$$

where $\underline{y} = (X_1, A_1, X_2, A_2, X_3, A_3)'$

$\underline{x} = (A_0, S, B)'$

$\underline{\Pi}_{yy} = (\pi_{y_i y_j})$, $i = 1, 2, \ldots, 6$; $j = 1, 2, \ldots, 6$

$\underline{\Pi}_{yx} = (\pi_{y_i x_j})$, $i = 1, 2, \ldots, 6$; $j = 1, 2, 3$

and

$\underline{\varepsilon} = (\varepsilon_{X_1}, \varepsilon_{A_1}, \ldots, \varepsilon_{A_3})'$.

$\underline{y}$ is the vector of endogenous variables and $\underline{x}$ the vector of exogenous variables. We define $\pi_{yiyi} = -1$ for all i; for $i \neq j$, π_{yiyj} is the (standardized direct) effect of y_j on y_i, and π_{yixj} is the effect of x_j on y_i. We postulate that $\pi_{yiyj} = 0$ for $j > i$, that is, that the effects are 'one-way', from a variable representing an event earlier in time to one representing a later event.

The maximum-likelihood (OLS) estimates for the standardized structural coefficients are reported in Table 4-1. We have not illustrated the unsaturated models with path diagrams, because they are intricate and less revealing of the patterns than is the table.

Table 4-1

Estimates of Significant Standardized Direct Effects and the Standardized Effects of the Disturbances Under the Model of Equation 4.1

Endogenous Variable	Predetermined Variable								
	School Experiences			Total Achievement			School Characteristics	Student Background	Disturbance (ε)
	Year 1	Year 2	Year 3	Fall Year 1	Spring Year 1	Spring Year 2			
Cohort 1									
School experiences, year 1	---	---	---	.327	---	---	.175	.272	.801
School experiences, year 2	.346	---	---	.000	.278	---	.134	.132	.723
School experiences, year 3	.230	.277	---	.000	.000	.207	.000	.116	.735
Total achievement, spring year 1	.212	---	---	.592	---	---	.000	.076	.639
Total achievement, spring year 2	.000	.173	---	.194	.559	---	.000	.000	.561
Total achievement, spring year 3	-.065	.000	.146	.073	.342	.433	.000	.051	.464
Cohort 2									
School experiences, year 1	---	---	---	.305	---	---	.160	.220	.854
School experiences, year 2	.265	---	---	.264	.000	---	.143	.202	.767
School experiences, year 3	.092	.422	---	.135	.000	.000	.124	.130	.721
Total achievement, spring year 1	.146	---	---	.698	---	---	.000	.117	.541
Total achievement, spring year 2	-.144	.154	---	.328	.495	---	.000	.099	.499
Total achievement, spring year 3	-.045	.000	.043	.105	.242	.565	.048	.000	.469
Cohort 3									
School experiences, year 1	---	---	---	.358	---	---	.118	.261	.801
School experiences, year 2	.280	---	---	.178	.195	---	.165	.132	.687
School experiences, year 3	.209	.359	---	.000	.000	.153	.070	.099	.699
Total achievement, spring year 1	.000	---	---	.782	---	---	.058	.112	.494
Total achievement, spring year 2	-.068	.126	---	.361	.472	---	.000	.056	.460
Total achievement, spring year 3	.095	.000	.066	.150	.244	.454	.000	.000	.431
Cohort 4									
School experiences, year 1	---	---	---	.392	---	---	.137	.198	.831
School experiences, year 2	.358	---	---	.000	.242	---	.000	.171	.776
School experiences, year 3	.126	.256	---	.000	.000	.225	.000	.154	.642
Total achievement, spring year 1	.065	---	---	.822	---	---	.000	.067	.452
Total achievement, spring year 2	.053	.000	---	.380	.480	---	.000	.095	.398
Total achievement, spring year 3	.000	.000	.000	.084	.105	.743	.000	.000	.404

Note. — The estimate of an effect is zero (indicated as .000) when the estimate under the saturated model was not significantly different from zero. In that case, the unsaturated model was adopted, with the corresponding parameter assumed to be zero.

The dashes (---) indicate that the parameter is not defined under the saturated model.

The extended model (4.1) resembles the basic one in that neither is subject to test; rather, both embody assumptions enabling interpretations of the correlation structure among the variables. On the other hand, the year-to-year representation of the schooling process raises issues that did not arise in the basic model and also highlights issues that were more easily ignored when considering that model.

First, we have not employed assessments of the students' summer-school or other educationally relevant experiences ~~of the students~~. Data on summer-school experiences are limited and STET ~~are~~ available in some detail for only a small, nonrandom subset of the sample and then for only the first year. In any event, such data indicate low levels of summer schooling and no effects of such schooling on growth in achievement (Klibanoff and Haggart, 1979).

Second, although fall measurements are available beyond the first year, only the spring measurements were used beyond fall 1976. In addition to simplifying the model and its interpretation, this tack seemed appropriate given the lack of data on summer experiences.

Third, and finally, when more than two (but only several) assessments of characteristics over time # -- # here achievement and school experiences # -- # are involved, there are generally two approaches to accounting fully for the correlations among the assessments, aside from the effects of other factors. In other words, there are generally two types of saturated models employed as the underlying framework.

One approach is simply an extension of the recursive model of (3.2) to (4.1), and specifies that each assessment may be dependent on all prior assessments, that is, that the entire history of achievement statuses prior to the current status is a determinant of that status. The second approach is to posit that the current status depends only on the immediately preceding status. Here, the assumption is that once the immediately prior status is known, there is no more information about the current status to be obtained by know~~n~~ing the 'path' that an individual took to get to the prior status. ~~Since~~ Because this approach involves fewer direct effects, it accounts for the correlation among achievement statuses by positing, in addition, that their disturbances may be correlated.

Each of these approaches has some appeal. On the one hand, whatever determinants of achievement statuses we have included in our model, it is doubtful that their residual influences (disturbances) are uncorrelated across test occasions. On the other hand, it seems reasonable also to suppose that the current status depends not simply on the previous one, but on one or several statuses prior to that as well. Unfortunately, we cannot incorporate both of these assumptions at once, unless we are prepared to assume that there are certain restrictions on the other structural parameters in the model (4.1). Otherwise, the model would be underidentified.

As indicated by Eq. (4.1), we have followed the first approach. For school experiences, it seems preferable to assume that the current experience may be dependent on several previous experiences and not just the immediately preceding one. For achievement status, the second approach was fully explored, but ~~was~~ found less satisfactory because the residuals between adjacent statuses had large negative correlations (ranging from -.187 to -.476). Mathematically, within a model with only three consecutive achievement statuses, say A_0, A_1, and A_2, a negative correlation between ε_{A_1} and ε_{A_2} is a necessary consequence of the fact that the correlation between A_0 and A_2 is greater than the product of the correlations between A_0 and A_1 and between A_1 and A_2. Even with the indirect influence of A_0 on A_1 through X_1, X_2, S, and B~~)~~ in our full model, a large, negative (unaccounted-for), residual correlation remains. The pattern of correlations between pairs of achievement statuses among A_0, A_1, and A_2 is substantively reasonable, but the meaning of the negatively correlated residuals for A_1 and A_2 is far from evident. In any event, whichever approach is used to represent the causal structure for the A's, the effects on the X's are (mathematically) identical, while the effects from the exogenous variables and X's to the endogenous A's are similar (for the present data). (The maximum-likelihood estimates under the rejected alternative family of models were obtained by the LISREL computer program [Jöreskog and Sörbom, 1978]. The final, unsaturated models were arrived at by a procedure similar to that described below for the adopted family of [recursive]models.)

The unsaturated models for which estimates are reported in Table 4-1 were arrived at by a slightly modified ~~kind of~~ backward-elimination procedure, with ~~slight modification~~. In general, in the first stage, we eliminated paths in each equation for which the t values were nonsignificant at the .25 level. In the second and third stages, we reduced the α level to .10 and .05, respectively. At each stage, log-likelihood ratio χ^2 statistics were calculated for the 'overall' evaluation of the current model against the saturated model and for the evaluation of the current model against the immediately prior one. The α level used was .05, except as noted below.

The above procedures were generally followed, except that preference was given to effects of predetermined variables that were closer in time to the current endogenous variable. For example, if the fit of the model was only modestly reduced by eliminating the effect of Λ_0 on X_2 instead of the effect of Λ_1 on X_2, when only one of these effects ~~was~~ would be sufficient, we eliminated the former effect. There were only two such cases where we departed from a strict backward-elimination procedure. In both cases, the increases in χ^2 were moderate over the 'standard' backward elimination model, ~~and~~ S although the difference in χ^2 statistics between the preferred model and the model of the previous stage was significant at the .025 level in each case, the χ^2 statistic for the final adopted model against the saturated underlying model was not significant at the .05 level in every case. For the four cohorts in order, the test statistics for the final models ~~are~~ were: #(1) $\chi^2_{10} = 14.23$, $p = .16$; #(2) $\chi^2_7 = 10.89$, $p = .14$; #(3) $\chi^2_7 = 8.95$, $p = .26$; and #(4) $\chi^2_{13} = 20.25$, $p = .09$.

Consistency of Results of the Extended and Basic Models. The patterns of effects for the elaborated model with year-to-year achievement and school experiences are almost completely consistent with the results reported in Chapter 3 for the basic model, where school experiences are averaged over the three years. To recapitulate:

(1) Among background, school quality, and prior achievement, the last is the most important determinant of the distribution of school resources. The effects of prior achievement, school quality, and unmeasured factors together indicate that school resources are assigned primarily on bases independent of social background. Additionally, the elaborated model indicates that resource distribution is generally dependent on the most recent achievement status.

(2) The direct effect of the student's background on his educational experiences is always appreciable, even after a history of experiences has been recorded. This fact was evident in Chapter 3 from the patterns of background effects on school experiences across the four cohorts observed. It is reinforced here by the intracohort patterns of continuing direct effects of background on the school experiences of students by grade. Thus, it appears that throughout $\overset{a}{\text{the}}$ student's educational career, there is a significant, continuing advantage in schooling received if he is of privileged background.

(3) The effect of school quality on achievement is primarily indirect, through the kinds of experiences and resources to which students are exposed in school. For cohort 2, there is a small direct effect on achievement in spring 1979, as

in the basic model. The only other exception is the direct effect of school quality on achievement in spring 1977 for cohort 3.

(4) The predominant determinant of later achievement is earlier achievement. Additionally, the elaborated model shows that achievement at any given point has a continuing, though diminishing influence over ~~the~~ a student's career. His two previous years' achievements are the most important influences on his current academic status. Achievement three years before, though always a significant factor, exerts a generally more modest influence than do his later statuses. It seems likely that were another year of data available, the effect of A_0 on A_4 would have been nonsignificant.

(5) The direct effects of social background on achievement are modest. Indeed, as more of ~~the~~ a student's academic history--in terms of both achievement and resource exposure--is known, a direct effect of background on achievement is not always reliably evident. Thus, the direct effects of background on spring 1979 achievement ~~in~~, evidenced in Chapter 3, are now seen as mediated in part by intervening achievements as well as by the disaggregated measures of school experiences. The patterns of background effects on achievement suggest that were more years of data available, the later effects of background on achievement would be largely indirect.

(6) The effects of school experiences are greatest in the earliest years of schooling and are substantial in those years in ~~by~~ comparison to the direct effects of background on achievement. As the student progresses through school, however, such effects

diminish to the point of showing no statistically reliable presence in the sixth grade for cohort 4. The effects, given when significant, in the second half of elementary schooling are either modest or are offset by the negative effects of highly correlated assessments of school experiences.

As in the case of previous results, we do not attribute much substantive meaning to these negative effects. The pattern is generally one of a positive impact of current school experiences on the next achievement assessment. The negative effect for some school-experience measures is in evidence only when current experiences have a significant, positive, and generally at least as large an effect on the same achievement status. The overall effect of schooling on a given achievement assessment, taking into account the offsetting negative effect, is positive, though modest. The total effect of the earlier experience assessment (with a negative direct effect) is either positive or just slightly negative and is, in the latter case, offset by the effect of that portion of the later assessment that is independent of the earlier assessment and of all other predetermined variables.

Inertia in the Distribution of Educational Resources. The model (4.1) brings one important phenomenon to light that was not revealed by the basic model, but ~~that~~ is implicit in the pattern of high and fairly stable correlations among ~~between~~ school experiences from year to year (Table C-2 of Appendix C). There is a great deal of inertia in the allocation of school resources, since those received beyond the first grade are distributed primarily on the basis of previous exposure levels. Indeed, roughly one-half of the correlation ~~between~~ among resource levels is due to the effects of the earlier level(s) on the current level, unmediated by intervening academic achievements. In addition, even allowing for some lag in the influence of academic achievement on ~~the~~ resources received, it is remarkable that the greatest direct effect on the current level of resources ~~from the level two years earlier is greater than the effect~~ from any earlier achievement assessment is from the level two years earlier. Finally, school-to-school differences cannot be the major reason for the stability of resource exposure, ~~since~~ because the direct effects of school quality on the student~~'s~~ experiences are in general substantially smaller than the autoregressive effects of experiences. It thus appears that administrative decisions on the allocation of resources, once made, are resistant to alteration in the light of later academic performance. While academic performance is taken into account in the allocation process, some weight is assigned in that process to prior resource levels, and hence~~, by the same fact,~~ previous performance is discounted.

Were schooling a more potent influence on the development of academic skills, the sheer inertia of the resource-allocation process might work only to sustain the background-achievement relationship by preserving the relative academic standings of students over time. However, as shown below, the stability of achievement is not the result of the inertia of the process, since schooling has only a modest influence on achievement.

Schooling and Achievement as a Feedback Process. As we indicated in Chapter 3, the background-achievement relationship is maintained over time mostly ~~in large part~~ because of the strong influence that earlier abilities exert on later achievement. The year-to-year effects of the model (4.1) confirm the indications of the basic model that the mechanism by which prior achievement is transformed into later achievement, is primarily direct and involves little cyclical feedback between achievement and schooling. The reason for the general absence of a feedback loop is that while achievement has a substantial impact on school experiences, those experiences have too small a net impact on achievement beyond the first two grades.

Table 4-2 indicates just how STET ~~little~~ academic abilities are transmitted, via ~~the~~ schooling received, to later achievement statuses. The table decomposes the total effect of fall 1976 achievement on achievement in each of the following three springs into the portions mediated by school experiences (X_i, $i = 1, 2, 3$) and the portions unmediated by schooling. Clearly, almost all of the total effect is unmediated by schooling. The schooling contribution is almost nil beyond the first two grades, except for cohort 3 in the fifth grade.

In short, any initial deficit in achievement, in large part due to background, is sustained almost completely by the large influence of earlier abilities on later achievement and depends to a small degree ~~little~~ on a weak positive feedback loop between achievement and schooling over time.

Table 4-2

Decomposition of the Estimated, Standardized Total Effects of Fall 1976 Achievement on Later Achievement: the Effects Mediated and Unmediated by Intervening School Experiences

Cohort	Later Achievement					
	Spring 1977		Spring 1978		Spring 1979	
	Unmediated	Mediated	Unmediated	Mediated	Unmediated	Mediated
1	.592	.069	.525	.052	.503	.019
2	.698	.045	.673	.009	.654	-.001
3	.782	.000	.730	.029	.672	.057
4	.822	.025	.774	.021	.746	.000

Directions for Further Research

The basic model of the schooling process was elaborated in two ways. First, we considered the differential effects of background and schooling on reading and math skills. Second, we followed the year-to-year progress of four cohorts over a three-year period. Further research in this area within the present context could follow at least two directions: construction of a synthetic-cohort model covering the entire elementary period and an analysis employing latent variables in 'linear structural relation' (LISREL) systems (Jöreskog, 1973, 1977; Jöreskog and Sörbom, 1978).

A Synthetic-Cohort Model. The construction of a synthetic-cohort model covering the entire elementary period is an obvious and natural extension of the model in this chapter. It would exhibit the influences of origins and schooling on achievement over a longer term, possibly magnifying any compounded or cumulative effects of earlier experiences and attributes as well as providing an additional, summary basis for determining the relative effects of schooling, ~~and the~~ home, and community in the earlier and later years.

While the idea of constructing a six-year synthetic-cohort model is appealing, it may be difficult to pursue with our data. Our own attempts have been unsuccessful. We will merely describe our efforts in some detail in the hope of stimulating creative solutions.

Table C-5 in Appendix C exhibits the sample data available for the six cohorts that were in elementary schools in the fall of 1976. The data are a consequence of the longitudinal design of the study, which followed each of the cohorts through completion of the 1978-9 school year or until graduation from the sixth grade, whichever occurred first. The table entries ~~in the table~~ are the cohorts providing data on elements of the covariance matrix that would be needed to construct a structural-equation model for the entire period. The variables involved are background (B), the six assessments of school experiences (X_1, X_2, . . ., X_6, where X_i = school experience in grade i), and the seven measurements of achievement (A_0, A_1, . . ., A_6, where A_i = achievement at the beginning of grade 1 if i = 0 and in the spring of grade i otherwise).

Table C-5 highlights two problems that any six-year synthetic-cohort model must address. First, some correlations (covariances) are unknown as a result of the 'incomplete' cohort-longitudinal design. There are two ways we may attempt to deal with this problem. One is to estimate the missing correlations by some means, as for example, use of independent data or interpolation based on adjacent values in the matrix. The second option is to impose certain restrictions on the structural coefficients before estimating them. Without these restrictions, there would be more coefficients to estimate than (known) correlations, and thus the coefficients would be underidentified.

The second problem highlighted by Table C-5 is that the correlations that are 'known', that is, for which estimates are provided by the data, are based on varying subsets of cohorts. Some decisions must be made as to which cohorts are to be used and how their estimates are to be combined.

In brief, our attempts at solving the problem of multiple estimates involved use of cohorts 1 and 4 alone to the extent possible. ~~with~~ other cohorts were used to minimize the number of unknown correlations, and ~~use of~~ all four cohorts were used in the three-year panel. (We did not use the data for cohorts 5 and 6.) The correlations constructed in each case were pooled within-cohort correlations.

To address the problem of missing correlations, we tried both options. Specifically, missing correlations were interpolated and the structural coefficients were then estimated, and, alternatively, restrictions were placed on the path coefficients in a recursive system (with uncorrelated disturbances) by specifying that direct effects involving variables separated by relatively long intervals of time, were nil. In both cases, an iterative approach was used; that is, the missing correlations were interpolated or derived from the constrained coefficients of each structural equation, moving forward in time. In this way, later filled-in correlations were based on earlier ones.

None of our combinations of approaches yielded satisfactory results. Either inadmissible or extremely implausible correlations or path coefficients were obtained in view of the patterns among the known correlations or among the coefficient estimates obtained in the three-year models. It appears that the patterns of relations among the variables are too inconsistent across the cohorts to construct a realistic model of any single cohort.

Measurement Errors. Another possible direction of research is perhaps fraught with fewer problems, ~~is~~ the consideration of errors of measurement in our indicators of achievement, background, and schooling. While we have occasionally alluded to measurement issues and measurement-error implications, we have not addressed them formally. Moreover, our approach involved the construction of composites rather than the use of latent variables and multiple indicators in LISREL models. Further work along those lines would indicate whether the essence of the conclusions drawn, if not the particular estimates obtained, are independent of measurement errors.

At this point, we may be entitled to take some comfort in the results obtained by Jencks et al. (1972: Appendix B) in models similar to Duncan's (1968). Both authors' models involved measures of social origins, early intelligence, educational attainment, later intelligence, and occupational and income attainments. In Jencks' work, the predetermined variables were both corrected and uncorrected for their unreliabilities. His results suggest that measurement errors that were ignored, at least to the extent that they were nonsystematic and uncorrelated, did not affect the conclusions drawn.

Conclusions

When the basic model is elaborated by including the intervening achievement levels between fall 1976 and spring 1979 and the disaggregated, annual assessments of school experiences, the results are almost completely identical to those obtained under the simple model. These results justify the initial, simpler approach to evaluating the influences of origins and schooling on achievement.

The elaborated model is useful, however, not only in confirming our earlier results, but also in demonstrating that the distribution of school resources over time depends primarily on the resources received earlier. This conclusion suggests that ~~were~~ if variations in school resources were more efficient in effecting differences in students' achievement levels, the background-achievement relationship would be further enhanced.

The elaborated model also shows emphatically that achievement differences prior to entry into school are maintained almost completely because earlier achievement strongly influences later achievement in a direct fashion, with an almost negligible contribution to the total effect from the feedback between schooling and achievement. While this feedback is positive, the portion of the 'loop' from schooling to achievement is so modest in value that over time the intervening school experiences contribute little to the creation of further achievement differences.

CHAPTER 5. EDUCATIONAL POLICY AND SOCIOECONOMIC ACHIEVEMENT

> The results of this study suggest that even if we were to increase the amount of educational resources available to students (of the same type as those studied) to levels near if not beyond the limits of practicability, we would not increase their academic skills by much nor significantly alter the background-achievement relationship. If such results are disturbing, they are counterbalanced by the fact that many other skills, personal qualities, and life events unrelated to intellectual and social origins and to prior educational and occupational achievements determine the economic success of individuals in our society.

There is a continuing tension in our society between the values of 'equal opportunity' and of differential rewards based on talent and effort. This is true in part because even as we seek to equalize opportunities, our intention is to allow individuals to advance as far as their abilities and efforts can take them and to enjoy the rewards that accrue from their achievements. One of these rewards is the ability of parents to provide material and other advantages to their offspring as the latter begin their own socioeconomic careers in the very first year of school.

To identify an appropriate set of educational policies, we need not ~~here~~--nor could we--settle here the competing claims ~~for~~ of egalitarianism and family ties in our society. However, the debate, carried on elsewhere, could benefit from a clearer understanding of the effects of origins and schooling on academic and adult socioeconomic achievement. That understanding, though still far from complete, has been greatly enriched by earlier studies of the socioeconomic life cycle--from social and intellectual origins through schooling to occupational and income attainments (Blau and Duncan, 1967; Duncan, 1968; Jencks et al., 1972).

The ~~conclusion of~~ these studies ~~is~~ conclude that educational attainment is the primary, direct, measured determinant of occupational status. That status in turn exerts the strongest direct influence on income. Social background and early intellectual skills are important determinants of educational attainment, but their effects on later achievements are primarily indirect, through the amount of education received. Hence, educational credentials are the key to occupational and economic success; early cognitive skills and social origins are primarily significant in increasing the likelihood of obtaining such credentials and are less important beyond school (once education has been taken into account).

The total effects of early intellectual ability on occupational and income attainments appear to be moderately strong by the best estimates currently available (ranging in standardized form from .19 to .29 in Duncan [1968] and Jencks et al. [1972~~: Appendix B~~]). Nevertheless, the primary factors accounting for economic success are independent of cognitive ability, as indicated by the large residuals of about .9 in the income equations in Duncan (1968) and Jencks et al. (1972). Consequently, there is almost as much income inequality among those who score high on standardized tests as in the general population (Jencks et al., 1972).

Contributing to this heterogeneity is the ~~fact that there is a~~ great ~~deal of~~ variation in income among those in the same occupation and the ~~fact that~~ dependence of occupational attainment ~~is dependent~~ on many skills and qualities, only a small number of which are measured by or correlated with standardized tests.

Given that it would take a large difference in achievement in elementary school to produce any significant change in adult income, one may question how important it is to attempt to reduce the association between academic achievement (as measured by standardized tests) and social background. However, regardless of how this normative question is resolved, there is also the empirical question of how much of an impact on the background-ability link we could hope to make by adopting 'optimal' educational policies, leaving alone the question of what specific characteristics those policies would possess.

The results of this study provide little ground~~s~~ for optimism to those who would seek to alter the background-ability relationship substantially. The effects of schooling on academic achievement, though appreciable in the early years, decline too rapidly and even at their maximum in the first grade are insufficient to transform that relationship. Of course, no correlational study--longitudinal or otherwise--can tell us what would occur if substantial increments in school resources were allocated to students because such prognostications would entail not only extrapolating beyond current ranges of experience, but almost certainly would also involve altering the correlations among the relevant variables on which depend the regression weights obtained herein ~~depend~~.

Nevertheless, we have no basis for believing that, were massive amounts of educational resources allocated, such resources would either be exploited by students or have increased effects on achievement. Moreover, even if we could sustain the effect of schooling at levels achieved in the early years of the elementary period *and* even if we were to eliminate all preferential treatment of socioeconomically privileged students (i.e., treatment not justified by their academic abilities), such changes would have almost no impact on the background-achievement nexus. That relationship is invariant from the start of schooling not primarily because the public education system discriminates in its allocation of resources, nor because the direct effects of background on subsequent achievement statuses are overwhelming (on the contrary, they are relatively modest), but rather because the effects of schooling are modest and are linked both directly and indirectly through prior achievement to background and because prior achievement itself is the primary determinant of later achievements.

If alteration of the background-ability connection seems impracticable, we may nevertheless seek to increase the opportunities of all students by increasing their academic skills. Again, however, the results of this study are not encouraging. We found the largest (metric) effect of school experiences on achievement in the three-year period observed for cohort 1 (Table C-3). Multiplication of that effect by the (metric) component coefficients for the school-experience components (Table 2-9) shows--if we believe the positive effects obtained--that if we were to increase the component values by large amounts near if not beyond the limits of practicality, we would increase achievement at the end of the third grade by less than two-fifths of a standard deviation. For cohort 4, the effect of the school-experience composite is only about one-fifth of that for cohort 1, so that the increase in achievement scores would be much less.

(The numerical details on which the result for cohort 1 is based ~~are a~~s follow~~s~~: The coefficients in the X composite for average achievement in homeroom [HR-CTBS]; proportion white or Asian in homeroom [HR-RACE]; average experience of teachers [TCHR-EXP], school attendance in weeks [WEEKS]; and hours of instruction attended in regular reading, regular math, and special math [REG-READ, REG-MATH, and SPL-MATH] are .1004, 3.139, .3438, .8731, .01551, .03140, and .05690, respectively. Suppose we increase these inputs by the following amounts [in order]: 80 [approximately two standard deviations], .2 [bringing the average proportion white near unity], 10 years [raising average TCHR-EXP from about 12 years to 22], 5 weeks [about two standard deviations], 100 hours [compared with a mean and standard deviation of about 180 and 90 in the second grade], 100 hours [compared with a mean and standard deviation of about 120 and 50 in the same grade], and 50 hours [compared with a mean and standard deviation of about 50 and 36 in the second grade]. The increase in third-grade achievement would be about 24 points, compared with a standard deviation of about 63, a difference of about 370 to 400 points between the highest and lowest scores in the third grade, and an average difference between the third and fourth grades of about 35 points.)

If such results are disturbing, it must be recalled that early academic skills are only a small determinant of economic success. The total (standardized) effect of educational attainment on income, independent of early intellectual skills and social background, is, by present estimates, anywhere from half as large to ~~being of~~ the same order of magnitude as the total (standardized) effect of early abilities. (Estimates of the former effects range from .14 to .19 in Duncan [1968] and Jencks et al. [1972].) More important, about 80 percent of the variation in income is unexplained by background, intellectual ability, educational attainment, or occupational status. It is, simply, ~~the case that~~ many other skills, personal qualities, and life events, unrelated to origins or ~~and~~ prior educational and occupational achievements, determine the economic success of individuals in our society.

Finally, our results do not imply that compensatory-education efforts, targeted at those at the margin of society, are ineffective or unimportant. We have examined the relationships among background, schooling, and achievement for the student population as a whole. An analysis of those relationships among those who are the chief recipients of compensatory education may reveal that such efforts can make a difference between remaining outside the mainstream of economic life and a greater probability of completing school or enough school to increase significantly the likelihood of holding down a steady job or otherwise more successfully participating in the society. If we do not seek to transform the background-achievement relationship for the society as a whole, but instead focus our efforts on reducing the numbers at the margin of society, those efforts may be extremely worthwhile.

APPENDIX A

THE SAMPLE

APPENDIX A

The Sample

The primary sample of the SES, known as the representative sample, consists of some 83,000 elementary students in 242 public schools with elementary grades. The sample of schools was drawn prior to the fall of 1976, although observations on the schools and students did not take place until that time. The schools were selected at random within 84 strata (usually three schools per stratum) defined by region, size, and poverty level of the school-district. Through appropriate stratification, schools in high-poverty districts were disproportionately selected. Within each selected school, all elementary students fell into the sample. In sampling terminology, the stet representative sample is a stratified (single-stage) cluster sample with clusters (schools) of unequal sizes selected with equal probabilities within each stratum.

The representative sample contains two subsamples that jointly provide the data for this study. The first subsample was drawn to gather background data from some students' parents. This subsample, drawn for a congressionally mandated survey and known as the sample for the participation study, consists of 15,579 students randomly selected from each of the 242 schools in the representative sample. The participation sample is a stratified two-stage sample with subsampling of (students from) the schools selected in the representative sample.

The second subsample, cross-cutting the first, was drawn for follow-up in the second and third years (1977-78 and 1978-79). The purposively selected subsample consists of students from 95 of the original 242 schools, except that students who left the elementary grades of the subsampled schools were not followed. The only exceptions to this procedure were a few students who, in the first year, attended some of the schools lacking the full span of elementary grades and later entered 'receiving' schools that had the complementary grades and ~~that~~ were purposively selected for follow-up of these students.

Sample Weights

There are several sets of weights that may be used for the study sample and longitudinal subsample, depending on the desired estimate of the population mean. The ratio estimate is generally preferred to the unbiased estimate for the kinds of characteristics of interest within the study (Cochran, 1963: sec. 9.12, 11.11), where the school means are less likely to depend on school size than are the totals. Previous projections from the representative sample used separate ratio estimates (Hoepfner et al., 1977), while those from the participation sample used combined ratio estimates (Breglio et al., 1978). Given the small number of clusters selected within each stratum, the combined estimate is generally preferred (Cochran, 1963: secs. 6.10-6.12). However, the separate estimate is used because there are no readily available data on the numbers of schools containing some of grades 1 to 4 by strata. Such data are necessary to develop the weights for the combined ratio estimate for the population of cohorts 1 to 4, (i.e., the cohorts in grades 1 to 4 in fall 1976) for three-year longitudinal analyses. Although the requisite data are available for obtaining the combined ratio estimate for the population of cohorts 1 to 6 for the first year of the study, the same type of estimate is used for cross-sectional and longitudinal analyses for consistency.

Although the separate estimate is used for convenience, the results of Table A-1 show that for the representative sample the combined and separate estimates of population characteristics in the 1976-7 school year are almost identical. Indeed, using equal weights--which tends STET to ignore the oversampling of disadvantaged students in the representative sample--again matters little, a fact of which we took advantage in some preliminary, 'unweighted' analyses.

Table A-1

Population Estimates of School, Background, and Achievement Characteristics from the Representative Sample: Grades 1 to 6, First Year

Characteristic	Combined Estimate		Separate Estimate		Unweighted Estimate	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Fall 1976 achievement	463.77	108.02	464.19	107.63	461.92	107.28
Receipt of reading CE service	0.23	0.42	0.23	0.42	0.22	0.42
Receipt of math CE service	0.16	0.37	0.16	0.37	0.15	0.35
Receipt of free lunch	0.32	0.47	0.33	0.47	0.37	0.48
Attendance in weeks	33.79	2.64	33.77	2.63	33.87	2.55
Experience of school's teachers	11.59	3.02	11.61	3.03		
Education of school's teachers	2.46	0.24	2.45	0.24	2.45	0.23
Principal's education	3.03	0.25	3.04	0.27	3.00	0.25
Race* (1=majority; 0=minority)	0.79	0.41	0.78	0.41	0.74	0.44
Mother's education*	3.08	0.97	3.07	0.97	3.07	0.98
Use of other language in home besides English*	0.93	0.25	0.93	0.25	0.92	0.28

* These data are based on teacher reports for all students in the representative sample. They are not based on the parent interviews within the sample of the participation study as are the data for race, mother's education, and the language spoken at home used outside this appendix. x stet

Note. — The numbers of cases on which the statistics are based range from 57,602 to 83,481.

These results and all others reported below for the representative sample, are based on an operational definition of that sample as consisting of the set of students in the selected schools with valid total achievement data in reading or math in the fall or spring of the 1976-7 school year (N = 83,481). While this definition is somewhat restrictive, it enabled the weighting analyses to be based on an existing data set containing most of the characteristics of interest (Wang et al., 1981). The same restriction does not apply to the participation sample (N = 15,579), so that it is possible that a case may appear in that sample and not in the representative sample as operationally defined, even though conceptually the former is a subsample of the latter.

The combined- and separate-estimate weights were obtained as follows. For the kth sample element (student) in school i of stratum h, the weight for the combined ratio estimate is N_h/n_h, where N_h and n_h are the numbers of schools in stratum h in the population and sample, respectively. The weight for the separate ratio estimate is $M_{oh}/(\sum_{i=1}^{N_h} M_{hi})$, where M_{oh} is the number of students in stratum h in the population and M_{hi} is the number of students in school i of stratum h in the population.

Because of the close agreement between the separate and combined estimates from the representative sample, there should be no objection to using the separate estimate as a standard in evaluating other samples and estimation procedures. Accordingly, Table A-2 shows that the much smaller participation sample is as representative of the population as is the representative sample, since the (separate ratio) estimates of the two samples are virtually identical. (In the case of the participation sample, for the kth sample element in school i of stratum h the weight is

$$\frac{M_{oh}}{n_h \sum_{i=1} M_{hi}} \frac{M_{ni}}{m_{hi}}$$

, where m_{hi} is the number of students in school i of ~~stratum h in the sample.)~~ — More this line downward to distinguish it from formula.

Table A-2

Separate Ratio Estimates of School, Background, and Achievement Characteristics from the Representative and Participation Samples: Grades 1 to 6, First Year

Characteristic	Representative Sample		Participation Sample	
	Mean	Standard Deviation	Mean	Standard Deviation
Fall 1976 achievement	464.19	107.63	463.77	107.13
Receipt of reading CE service	0.23	0.42	0.23	0.42
Receipt of math CE service	0.16	0.37	0.17	0.37
Receipt of free lunch	0.33	0.47	0.32	0.47
Attendance in weeks	33.77	2.63	33.83	2.52
Experience of school's teachers	11.61	3.03	11.59	3.02
Education of school's teachers	2.45	0.24	2.44	0.24
Principal's education	3.04	0.27	3.04	0.28
Race* (1=majority; 0=minority)	0.78	0.41	0.78	0.42
Mother's education*	3.07	0.97	3.09	0.97
Use of other language in home besides English*	0.93	0.25	0.93	0.25

*These data are based on teacher reports for all students in the representative sample. They are not based on the parent interviews within the sample of the participation study as are the data for race, mother's education, and the language spoken at home used outside this appendix. X ~~Add~~

Note. — The numbers of cases ~~on which the~~ statistics from the participaton sample are based range from 11,928 to 15,550.

Sample Representativeness: Nonrandom Selection and Attrition

Developing weights for the three-year panel was more problematic. The panel is first a result of a purposive (nonprobabilistic) selection of 95 of the original 242 schools, of which only 92 contain some of grades one to four. Within each stratum, preference was informally given to complete schools with summer sessions (at the schools or nearby) and with low-achieving and poor students (Hoepfner, 1981). Second, most transfer students were not followed after the fall of 1976. Such students have been found to differ in background and achievement from students who would otherwise have remained in the panel (Zagorski et al., 1981). It is likely that the nonrandom pattern of self-selection out of the study, present from its inception, had more significant consequences as the number of nonparticipants increased over the years.

Given the results of Table A-2, it appears sufficient to examine the effects of the purposive selection of 95 schools for the longitudinal study and of attempts to correct for this selection within the representative sample alone. Comparison of ~~cols.~~ columns 2 to 3 with ~~cols.~~ columns 4 to 5 of Table A-3 reveals that, as intended by the purposive selection, the subsample selected for second- and third-year follow-up is consistently more disadvantaged academically and socioeconomically in relation to the (first-year) cross-section sample. (The differences are completely unaffected by attrition, since only first-year characteristics are estimated.) The estimates from the

Table A-3

Separate Ratio Estimates of School, Background, and Achievement Characteristics from the Full Representative Sample and the Longitudinal Subsample Assuming Random Selection of Schools and Stratified Random Subsampling of Schools: Grades 1 to 6, First Year

Characteristic	Full Sample		Random Selection of Schools		Stratified Random Subsampling of Schools	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Fall 1976 achievement	464.19	107.63	454.41	106.47	462.59	105.56
Receipt of reading CE service	0.23	0.42	0.29	0.45	0.27	0.45
Receipt of math CE service	0.16	0.37	0.22	0.41	0.21	0.41
Receipt of free lunch	0.33	0.47	0.37	0.48	0.37	0.48
Attendance in weeks	33.77	2.63	33.83	2.66	33.86	2.62
Experience of school's teachers	11.61	3.03	11.51	3.06	11.51	3.06
Education of school's teachers	2.45	0.24	2.44	0.27	2.44	0.27
Principal's education	3.04	0.27	3.01	0.19	3.01	0.19
Race* (1=majority; 0=minority)	0.78	0.41	0.74	0.44	0.74	0.44
Mother's education*	3.07	0.97	3.03	0.96	3.04	0.95
Use of other language in home besides English*	0.93	0.25	0.93	0.25	0.93	0.25

*These data are based on teacher reports for all students in the representative sample. They are not based on the parent interviews within the sample of the participation study as are the data for race, mother's education, and the language spoken at home used outside this appendix.

X Att

longitudinal sample in ~~cols~~ columns. 4 to 5 consistently underrepresent the academic achievement and socioeconomic backgrounds of the student population because the weights assume a random selection of schools, whereas in fact the schools were purposively selected to overrepresent students at the lower ends of the continua. Also, the discrepancies between the two sets of estimates on achievement and background variables are significantly larger than in previous comparisons. On the other hand, it should be noted that the discrepancies are rather small in standard deviation units--roughly about .1, regardless of the estimate of the standard deviation used. The two samples--cross-sectional and longitudinal--are not all that different. One is unlikely to form a substantially different impression about the population from the longitudinal sample than from the cross-section, despite the purposive selection of follow-up schools. (A similar conclusion is drawn by Hoepfner [1981] in comparing estimates primarily of school [rather than student] characteristics.)

Although the differences between the longitudinal and cross-section samples are not large, an attempt was made to correct for the nonrandom selection of longitudinal schools by developing a set of weights that better reflected the preference given to certain types of schools. These weights were based on a premise that, though no less fictional than the assumption of random selection of schools, was thought might yield more accurate estimates. The premise is that the overrepresentation of certain characteristics in the longitudinal sample is due to a disproportionately stratified random sampling within all first-year schools by achievement, socioeconomic background, and so forth, rather than subselection of schools whose students tend to have certain achievement levels, backgrounds, and other characteristics.

Sample sizes within the strata prevented substratification beyond a division of total achievement scores into quartiles and a 'missing data' category. Within some strata, even the quartile division had to be collapsed. Thus, other relevant substratifying dimensions such as race, grade, parents' education, compensatory-educational services, and access to summer programs were not considered and a finer subdivision of the achievement dimension was not feasible. Columns 6 to 7 of Table A-3 report separate ratio estimates based on the assumptions of stratification of the students in the cross-section schools by total achievement scores and use of the same student subsampling fraction within a given substratum across all the cross-section schools in a stratum. (The subsampling fraction is $[\sum_{i=1}^{n_h} m_{hij}]/[\sum_{i=1}^{n_h} M_{hij}]$ for the jth substratum, where n_h is the number of cross-section schools and M_{hij} and m_{hij} are the population and longitudinal sample sizes in substratum j of school i, stratum h. The weight for the kth sample element within substratum j of school i, stratum h is $\frac{M_{oh}}{\sum_{i,j} M_{hij}} \frac{\sum_i M_{hij}}{\sum_i m_{hij}}$.)

As one would expect, the correction removes most of the underestimation of the CTBS achievement scores assuming a random selection of longitudinal schools (compare ~~cols.~~ columns 4 and 6). However, there is a disappointingly insufficient improvement in the estimates of the proportion that is white and the proportions receiving compensatory services. Given the smaller size of the longitudinal subsample of the participation sample compared to that of ~~than of~~ the representative sample, even less correction would be feasible in the smaller sample. Thus, considering both the small improvement arising from the more complicated weights and the tolerable underestimation of academic and background characteristics entailed by using weights that simply assume a random selection of longitudinal schools, analyses of the participation sample are based on the simpler procedure, with the weights proportionally adjusted to sum to the number of students in cohorts 1 to 4 selected for follow-up, namely, 4,774 (of which only 2,966 remained at the end of the longitudinal study).

Finally, Table A-4 shows that attrition of the longitudinal subsample of the participation sample, resulting from departure of students before termination of the study, did not dramatically alter the composition of the final sample. (The participation

Table A-4

Separate Ratio Estimates of School, Background, and Achievement Characteristics from the Participation Study Sample and the Longitudinal Subsample Before and After Attrition: Grades 1 to 4, First Year

Characteristic	Cross-Section Sample		Longitudinal Subsample			
			Before Attrition		After Attrition	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Fall 1976 achievement	414.29	87.05	411.61	86.04	413.52	86.01
Receipt of reading CE service	0.25	0.43	0.30	0.46	0.32	0.47
Receipt of math CE service	0.18	0.38	0.21	0.41	0.23	0.42
Receipt of free lunch	0.33	0.47	0.41	0.49	0.37	0.48
Attendance in weeks	33.75	2.52	33.86	2.52	33.90	2.40
Experience of school's teachers	11.58	3.04	11.56	3.05	11.73	3.10
Education of school's teachers	2.44	0.24	2.44	0.27	2.44	0.26
Principal's education	3.06	0.30	3.01	0.19	3.01	0.19
Race[*] (1=majority; 0=minority)	0.77	0.42	0.74	0.44	0.76	0.43
Mother's education	2.94	1.21	2.87	1.18	2.89	1.14
Use of other language in home besides English	0.93	0.25	0.93	0.25	0.94	0.24

[*] These data are based on teacher reports for all students in the representative sample. They are not based on the parent interviews within the sample of the participation study as are the data for race used outside this appendix.

Note. — The numbers of cases on which the statistics from the cross-section are based range from 9,980 to 10,110. The range for the longitudinal subsample before attrition is 4,722 to 4,774; after attrition 2,956 to 2,966.

sample was used instead of the representative sample for the comparisons of Table A-4 because data on the status of students in the study after the first term were not readily available for the larger sample.)

The three sets of statistics in ~~cols.~~ columns 2 to 7 are estimates for (1) cohorts 1 to 4 in the entire participation sample, (2) the longitudinal subsample of cohorts 1 to 4 selected for follow-up for two years, and (3) the remainder of the subsample after attrition. In short, when weighted to obtain separate ratio estimates from the participation sample, the entire sample and longitudinal subsample after attrition yield estimates acceptably close to the best available estimates of population characteristics.

In conclusion, the weights constructed for the three-year panel are similar to those developed for the cross-section; that is, they assume that the 92 schools were randomly selected within the strata and do not correct for nonrandom attrition. Though not strictly appropriate, given the nonprobabilistic design of the panel, the weights at least reflect the unequal sizes of and disproportionate sampling from the strata. Moreover, as shown above, neither the nonrandom selection of schools for the longitudinal study nor the pattern of attrition over the years appears to have seriously biased our estimates of population means and variances based on these weights.

APPENDIX B

COHORT PATTERNS OF BACKGROUND, SCHOOLING, AND ACHIEVEMENT

APPENDIX B

Cohort Patterns of Background, Schooling, and Achievement

The most important conclusion that can be drawn from an examination of the backgrounds, school experiences, and achievements of the four cohorts in the three-year panel is that they are essentially alike, not only in their origins, but also in their schooling and achievement as they proceed through the elementary grades. This is an important result, though hardly surprising given the short span of years covered by these cohorts at their points of entry into school. It enables us to consider what differences are found among the cohorts to reflect simply their different ages (grades) at any given time.

As Table B-1 shows, the four cohorts come from similar socioeconomic backgrounds. The average child's father and mother have completed (or nearly completed) high school and the family's total income was about $13,000 in 1976. (Code categories for some of the variables are given in Table C-1.) About one-fourth of the students are of minority backgrounds (neither white, Asian, nor non-Hispanic) and about one-sixth come from single-parent homes.

The most striking patterns in Table B-1 are in respect to the number of books available in the home for the child at his reading level and the percent of parents who attended some school

Table B-1

Means and Standard Deviations of Selected Background Characteristics by Cohort

Background Characteristics	Cohort 1		Cohort 2		Cohort 3		Cohort 4	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Father's education (FATHER)	2.99	1.33	3.03	1.33	3.14	1.45	3.05	1.46
Mother's education (MOTHER)	2.83	1.15	2.96	1.10	2.94	1.21	2.92	1.12
Occupation of household head (OCC)	2.30	1.37	2.45	1.46	2.44	1.47	2.48	1.61
Family income (INCOME)	6.67	3.28	7.05	3.46	6.74	3.69	7.33	3.60
Race/ethnicity (RACE)	.75	.43	.78	.42	.75	.45	.78	.45
Presence of 2 parents (2PARENTS)	.84	.36	.86	.35	.84	.38	.86	.38
Parents' attendance at school events (ATTEND)	.71	.45	.65	.48	.66	.49	.63	.54

Note. — Each statistic is based on all cases in the three-year panel for which data are available for the variable involved.

event in the first year. Since there are no data beyond the first year, it is possible that the differences for BOOKS and ATTEND are true cohort differences, but it is much more likely that they simply reflect age/grade differences. With respect to the parents' attendance at school events, it appears that an initial interest on the part of parents in the first year of their child's schooling diminishes as he progresses through school. As for the number of books, it could be that parents provide more reading materials for their child as he progresses through school or that, as his reading ability increases, more books become accessible to him. Perhaps the latter consideration primarily accounts for the largest intercohort difference, that between cohorts 1 and 2.

As in the case of their backgrounds, the characteristics of the students' schools are similar across the cohorts. Indeed, it would be surprising if the opposite were true, since as the school-level characteristics are identical across the grades within a school. Thus, the only factor contributing to grade differences on school-level characteristics is the fact that the schools differ somewhat in their grade distributions. As for school-grade-level characteristics, the average racial composition of each cohort's school-and-grade is virtually identical to its racial composition in the sample. The sole intercohort differences lie in achievement levels and are clearly due to grade differences (see Table B-3).

The classroom environments and individual experiences of the cohorts as they progress through school exhibit substantial similarity, as illustrated in Table B-2. (In this and the remaining two tables, cohorts 5 and 6 are included to provide additional data for grades 5 and 6.) The classroom environments of students are essentially constant within and across the cohorts. The quality of the students' teachers as gauged by professional experience alone and the students' attendance at school are similarly constant. However, after a peak is reached in the second grade, fewer and fewer students in general are assigned to compensatory-education programs. The lower participation in grade 1 than in grade 2 may be a function of the time it takes for school personnel to judge the need for such programs among newly entering cohorts. The decline in participation after grade 2 probably reflects an emphasis on compensatory education in the earlier grades. Similarly, students receive fewer hours of reading instruction as they progress through school, while there is no clear-cut pattern for math. In the sixth grade, the number of hours of reading instruction is reduced to about 60 percent of that received in the first grade.

Table B-2

Means and Standard Deviations of Selected Schooling Variables by Cohort

Variables	Grade	Means Study Year 1	 2	 3	Standard Deviations Study Year 1	 2	 3
Average achievement in homeroom, fall year 1 (HR-CTBS)	1	317	---	---	24	---	---
	2	398	399	---	31	34	---
	3	456	460	458	34	34	33
	4	501	507	509	36	38	39
	5	541	546	547	38	37	49
	6	578	586	588	36	41	46
Proportion white or Asian in homeroom, year 1 (HR-RACE)	1	.778	---	---	.32	---	---
	2	.791	.739	---	.32	.34	---
	3	.782	.749	.756	.33	.34	.33
	4	.780	.740	.776	.32	.35	.32
	5	.778	.764	.766	.33	.35	.35
	6	.821	.737	.789	.28	.33	.36
Average experience of teachers (TCHR-EXP)	1	11.9	---	---	8.2	---	---
	2	12.2	11.0	---	7.8	6.9	---
	3	12.0	12.0	11.8	7.1	7.6	6.7
	4	12.3	11.3	11.0	7.1	7.1	6.4
	5	12.0	12.9	13.5	6.6	7.1	6.7
	6	10.8	11.2	10.9	6.3	5.9	7.3
Participation in compensatory education program(s) (CE)	1	.387	---	---	.73	---	---
	2	.465	.442	---	.74	.71	---
	3	.420	.388	.384	.75	.69	.64
	4	.448	.379	.342	.75	.71	.66
	5	.376	.355	.339	.69	.74	.67
	6	.312	.372	.245	.64	.71	.60
Attendance in weeks (WEEKS)	1	33.5	---	---	2.5	---	---
	2	33.8	34.1	---	2.4	2.1	---
	3	33.9	34.1	34.6	2.5	2.4	1.7
	4	33.9	34.2	34.7	2.6	2.4	1.7
	5	34.1	34.3	34.7	2.3	2.3	1.9
	6	33.9	34.0	34.8	2.5	2.6	1.9
Hours of regular reading instruction attended (REG-READ)	1	194	---	---	101	---	---
	2	178	177	---	96	86	---
	3	146	150	156	76	77	74
	4	134	138	137	66	61	56
	5	127	131	140	62	65	70
	6	114	129	126	53	53	49
Hours of special reading instruction attended (SPL-READ)	1	111	---	---	75	---	---
	2	113	109	---	79	66	---
	3	85	95	99	65	74	69
	4	67	72	76	55	61	60
	5	64	69	65	53	61	56
	6	57	58	53	48	50	53
Hours of regular math instruction attended (REG-MATH)	1	121	---	---	56	---	---
	2	118	126	---	46	51	---
	3	118	130	130	38	44	45
	4	120	133	126	45	46	41
	5	120	122	126	47	44	49
	6	120	117	128	43	44	38
Hours of special math instruction attended (SPL-MATH)	1	47	---	---	34	---	---
	2	53	45	---	38	35	---
	3	47	48	51	37	37	37
	4	48	46	39	40	44	39
	5	49	48	50	44	43	43
	6	40	47	39	37	39	34

Note. — All statistics, with the exception of those in the first study year for grade 5 and in the first and second study years for grade 6, are based on the three-year panel. In the first two exceptional cases, the statistics are based on the cross-section; in the last case, on the unweighted two-year panel. In every case, only those students with data on all of the above variables and on the fall and spring total achievement scores in a given year were included in the statistics for that year.

More important than the between-grade pattern for reading is the tremendous variation in instructional hours in both reading and math within each grade. This variation is particularly true and to be expected for special instruction, ~~since~~ because not all students receive such instruction. However, for regular instruction, the standard deviation is often one-half or one-third as large as the mean number of hours within a grade. To a certain extent, variations in instructional hours are due to the generally negative relationship between the regular and special instructional components (Table 2-8). However, even when the total number of hours is considered, the standard deviation is still about one-fourth to one-third as large as the mean.

Finally, Table B-3 shows that the achievement levels and academic heterogeneity of the cohorts are more or less similar as they pass through any given grade. (Achievement scores range from a

Table B-3

Means and Standard Deviations of Total Achievement by Study Year and Grade

Study Year	Grade[1]						
	0	1	2	3	4	5	6
Means							
1	315	394	458	510	542	579	614[2]
2	---	---	456	511	548	584	615[3]
3	---	---	---	507	548	590	624
Standard Deviations							
1	36	50	55	63	64	69	72[2]
2	--	--	56	59	63	69	74[3]
3	--	--	--	64	61	73	76

[1] The first entry in each row corresponding to the first study year is based on the fall 1976 measurement for cohort 1. This entry occurs under grade "0" and is intended to represent the level of achievement prior to entrance into school. All other entries are based on the spring measurements.

[2] Based on the cross-section sample

[3] Based on the unweighted two-year panel.

Note. — Unless otherwise noted, each statistic is based on the three-year panel.

minimum of 207 to a maximum of 822 across the six grades and three study years. A mean of 500 and a standard deviation of 60 for cohort 4 in the fall of 1976 was fixed in developing the vertical scale scores in the main SES sample [Hemenway et al., 1978: 20].) The table also indicates (along the diagonal) an increase in ~~that~~ ~~the~~ variance as well as in the level of achievement for ~~within~~ a given cohort ~~increases~~ as it advances through school. If measurement error variance were constant over the entire CTBS scale, we would expect this to translate into increased reliability of the scale with advancing grade level. The pattern of correlations between test administrations is consistent with this expectation. As Table B-4 shows, the intertest correlations tend to increase as we consider later grade levels within a cohort or compare cohorts in any given year. Thus, the lower bounds on the reliabilities of the test increase with grade level.

Table B-4

Correlations of Total Achievement at One-, Two-, and Three-Year Intervals by Cohort

Test Interval	Cohort 1	2	3	4	5	6
One-Year Interval						
Fall 1976 - Spring 1977	.74	.84	.86	.89	.90[1]	.91[1]
Spring 1977 - Spring 1978	.82	.82	.86	.89	.90[2]	--
Spring 1978 - Spring 1979	.85	.86	.88	.91	--	--
Two-Year Interval						
Fall 1976 - Spring 1978	.71	.80	.85	.88	.90	--
Spring 1977 - Spring 1979	.81	.80	.86	.85	--	--
Three-Year Interval						
Fall 1976 - Spring 1979	.70	.76	.84	.82	--	--

[1] Based on the cross-section sample.

[2] Based on the unweighted two-year panel.

Note. — Unless otherwise noted, each correlation is based on the three-year panel.

Of course, the same pattern is consistent with unchanging reliability of the achievement scale and increasing stability in (true) achievement within grades (actually, from spring to spring in most cases in the table) as students advance through the grades. Finally, the pattern is consistent with a combination of increased reliability and stability, an interpretation for which some evidence is provided in Chapter 3. Whatever the underlying factor(s), the increasing correlations and the singularly low intertest correlation in the first grade should be kept in mind as we examine the models of the later chapters.

In summary, the four cohorts exhibit substantial similarity in their backgrounds, schooling experiences, and achievement as they pass through the elementary grades. This result enables us to consider any observed differences among the four cohorts in their educational careers in any year as a function simply of grade level. Differences between grade-level experiences occur, but not in terms of the peers or teachers with whom students interact. Grade differences occur primarily in terms of the receipt of compensatory-educational services and the amount of reading instruction.

APPENDIX C

SUPPLEMENTARY TABLES

Table C-1

Description of Some of the Variables Appearing in the Background (B), School-Characteristics (S), and School-Experience (X) Composites

Variable Abbreviation	Code	Description of Variables and Code Categories
FATHER		Father's educational attainment
	0	Family does not have a father in the house
	1	8th grade or less
	2	9th through 11th grade
	3	12th grade
	4	Some college
	5	College graduate
	6	Graduate or professional school
MOTHER		Mother's educational attainment
	0	Family does not have a mother in the house
	1	8th grade or less
	2	9th grade through 11th grade
	3	12th grade
	4	Some college
	5	College graduate
	6	Graduate or professional school
OCC		stet Occupation of the household head (the father if present and employed, the mother otherwise)
	1	Operatives, unskilled and farm laborers, service and private household workers
	2	Craftsmen
	3	Sales and clerical workers
	4	Managers and administrators, including farmers, farm managers, and armed service officers
	5	Professional and technical workers
INCOME		Total family income in intervals
	0	None
	1	1 — 2,500
	2	2,501 — 5,000
	3	5,001 — 7,000
	4	7,001 — 9,000
	5	9,001 - 11,000
	6	11,001 - 13,000
	7	13,001 - 15,000
	8	15,001 - 17,000
	9	17,001 - 19,000
	10	19,001 - 21,000
	11	21,001 - 24,000
	12	24,001 - 29,000
	13	29,001 and over
RACE		Race/ethnicity of the parent respondent
	0	Black, native Indian or Alaskan, or of Hispanic origin
	1	White, Asian, or Pacific Islander, and not of Hispanic origin
2PARENTS		Number of parents present in the home
	0	Only one parent
	1	Both parents
BOOKS		Number of books in the home that the child can read
	0	0 (None)
	1	1 - 10
	2	11 - 20
	3	21 - 30
	4	31 - 40
	5	41 - 50
	6	51 and over
ATTEND		Parents' attendance at school events in the first study year
	0	No events attended
	1	One or more events attended
TRAINING		Level of compensation given teachers for inservice training
	1	Have no inservice training programs
	2	Staff attend on own time and are not paid for attendance
	3	Staff are released from regular assignment, or staff attend on own time and are paid to attend
LIBRARY		Presence of central library at school
	0	No
	1	Yes
PRINCIPAL		Principal's educational attainment
	0	No degree
	1	Degree or diploma based on less than 4 years' work
	2	Bachelor's degree
	3	Master's degree, or bachelor's degree plus 5th-year preparation, or six-year specialist degree
	4	Doctor's degree
CE		Participation in compensatory-education program in reading and/or math
	0	Does not participate
	1	Participates

Note. — Missing values are excluded from the descriptions of the codes.

Table C-2

Means, Standard Deviations, and Correlations Among Achievement, School-Experience, School-Characteristics, and Background Variables

	Cohort 1			Cohort 1 (below diagonal) and Cohort 2 (above diagonal)																		Cohort 2		
	Mean	S.D.	N	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Mean	S.D.	N
1. Total achievement, fall year 1	315.40	37.13	855		.82	.80	.76	.90	.88	.77	.73	.77	.67	.73	.71	.60	.45	.51	.48	.23	.49	396.34	47.71	789
2. Total achievement, spring year 1	390.65	47.47	845	.74		.82	.80	.77	.70	.92	.89	.80	.65	.77	.71	.61	.51	.51	.46	.26	.52	455.10	53.46	783
3. Total achievement, spring year 2	455.92	56.04	841	.70	.80		.86	.76	.70	.80	.70	.93	.87	.82	.78	.56	.37	.55	.49	.26	.53	512.24	57.19	771
4. Total achievement, spring year 3	506.05	64.13	797	.69	.81	.83		.74	.66	.79	.67	.83	.70	.95	.89	.55	.37	.50	.48	.28	.50	547.94	61.40	745
5. Reading achievement, fall year 1	347.10	33.10	857	.95	.69	.65	.64		.63	.81	.60	.76	.59	.74	.64	.57	.43	.51	.45	.21	.53	418.27	47.41	790
6. Math achievement, fall year 1	333.29	35.16	855	.87	.70	.67	.66	.70		.59	.72	.63	.66	.59	.68	.49	.34	.41	.43	.20	.35	398.23	43.66	790
7. Reading achievement, spring year 1	411.39	43.62	848	.70	.92	.75	.79	.67	.64		.66	.84	.56	.81	.65	.61	.50	.51	.45	.23	.55	462.72	53.01	786
8. Math achievement, spring year 1	393.90	43.10	848	.65	.90	.72	.70	.58	.66	.68		.61	.64	.59	.67	.48	.40	.41	.37	.21	.39	453.93	48.57	785
9. Reading achievement, spring year 2	461.12	55.47	849	.66	.77	.93	.83	.62	.63	.76	.64		.66	.84	.69	.57	.42	.54	.46	.25	.53	504.03	55.58	779
10. Math achievement, spring year 2	458.13	48.58	842	.61	.69	.88	.70	.55	.62	.60	.68	.68		.61	.72	.46	.22	.48	.45	.21	.39	515.82	55.80	773
11. Reading achievement, spring year 3	500.32	61.33	805	.66	.78	.81	.95	.63	.62	.79	.64	.85	.61		.72	.52	.37	.47	.44	.25	.50	534.22	62.61	749
12. Math achievement, spring year 3	509.27	57.89	804	.64	.75	.74	.92	.58	.62	.70	.67	.68	.70	.77		.53	.33	.49	.47	.29	.43	557.81	60.52	747
13. Three-year total school experiences	181.44	17.23	781	.61	.66	.66	.66	.60	.54	.66	.55	.66	.53	.68	.55		.78	.85	.84	.38	.53	197.48	16.73	693
14. School experiences, year 1	53.77	6.55	849	.51	.55	.53	.50	.51	.47	.53	.48	.54	.41	.53	.41	.84		.51	.45	.28	.41	60.52	6.87	784
15. School experiences, year 2	60.85	7.27	843	.52	.57	.59	.58	.52	.46	.57	.48	.58	.49	.58	.48	.85	.61		.64	.33	.47	66.26	7.20	779
16. School experiences, year 3	66.30	6.59	797	.48	.53	.55	.59	.48	.42	.55	.41	.55	.44	.60	.49	.84	.56	.59		.35	.46	70.48	6.26	712
17. School characteristics	-54.81	5.49	857	.27	.24	.22	.23	.29	.21	.22	.22	.22	.15	.25	.18	.37	.33	.35	.24		.24	-53.12	4.95	793
18. Student background	27.98	10.92	817	.51	.48	.47	.49	.48	.47	.48	.42	.49	.35	.52	.40	.54	.49	.47	.45	.26		30.99	11.76	754

	Cohort 3			Cohort 3 (below diagonal) and Cohort 4 (above diagonal)																		Cohort 4		
	Mean	S.D.	N	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Mean	S.D.	N
1. Total achievement, fall year 1	453.77	53.60	755		.89	.88	.83	.95	.92	.87	.77	.86	.76	.84	.67	.62	.50	.50	.59	.10	.50	507.14	64.30	557
2. Total achievement, spring year 1	509.35	60.69	749	.86		.89	.84	.87	.80	.95	.90	.88	.77	.84	.69	.62	.51	.51	.57	.09	.50	549.99	66.67	550
3. Total achievement, spring year 2	550.61	65.06	738	.84	.86		.91	.86	.78	.87	.78	.95	.91	.89	.77	.64	.53	.51	.61	.13	.55	589.43	71.81	543
4. Total achievement, spring year 3	589.86	72.62	713	.83	.85	.87		.81	.74	.83	.72	.88	.81	.94	.88	.59	.48	.47	.56	.09	.53	624.47	76.03	519
5. Reading achievement, fall year 1	466.38	54.63	755	.93	.85	.83	.82		.76	.89	.69	.88	.69	.85	.61	.62	.50	.50	.59	.10	.51	505.10	64.30	557
6. Math achievement, fall year 1	447.06	46.30	756	.89	.73	.71	.70	.69		.73	.76	.72	.75	.72	.64	.55	.46	.42	.52	.09	.42	505.85	60.93	557
7. Reading achievement, spring year 1	503.99	57.39	753	.84	.94	.83	.82	.88	.65		.73	.90	.69	.86	.63	.60	.48	.51	.56	.08	.52	534.48	68.78	553
8. Math achievement, spring year 1	511.55	57.98	750	.74	.90	.75	.75	.66	.70	.73		.70	.77	.67	.67	.57	.47	.44	.51	.10	.41	563.09	66.12	552
9. Reading achievement, spring year 2	533.43	63.86	744	.81	.83	.95	.85	.84	.64	.84	.67		.75	.91	.69	.62	.50	.50	.60	.10	.55	566.98	70.45	547
10. Math achievement, spring year 2	564.38	67.54	741	.76	.78	.91	.79	.70	.70	.71	.75	.75		.73	.78	.58	.48	.46	.55	.15	.47	606.89	71.05	545
11. Reading achievement, spring year 3	567.66	71.37	714	.79	.82	.85	.95	.82	.62	.83	.68	.88	.70		.70	.59	.47	.48	.57	.09	.56	597.22	74.72	520
12. Math achievement, spring year 3	607.12	73.32	715	.75	.74	.76	.90	.68	.70	.67	.72	.67	.79	.73		.49	.43	.39	.46	.06	.41	641.12	74.69	523
13. Three-year total school experiences	209.74	18.51	691	.66	.63	.63	.67	.65	.54	.65	.55	.62	.59	.64	.63		.89	.80	.87	.18	.53	220.68	18.18	488
14. School experiences, year 1	64.75	7.73	754	.54	.49	.46	.54	.55	.42	.52	.38	.46	.42	.51	.52	.85		.55	.65	.22	.42	69.09	8.32	553
15. School experiences, year 2	70.19	6.85	749	.61	.61	.63	.63	.58	.53	.60	.55	.61	.58	.61	.57	.86	.59		.62	.17	.44	74.10	6.38	552
16. School experiences, year 3	74.20	7.10	702	.55	.56	.55	.59	.53	.46	.55	.51	.54	.52	.56	.55	.85	.56	.66		.14	.53	76.92	6.52	496
17. School characteristics	-52.78	5.67	759	.25	.29	.27	.29	.24	.19	.29	.25	.28	.23	.30	.26	.41	.29	.39	.34		.21	-51.13	3.58	557
18. Student background	31.72	12.64	721	.58	.58	.57	.59	.59	.45	.61	.49	.56	.51	.59	.50	.61	.51	.54	.51	.32		32.27	12.26	528

Table C-3

Estimates of Metric Effects for the Models of Figure 3-2

Effect	Endogenous Variable	Predetermined Variable			
		Three-Year Total School Experiences	Total Achievement Fall Year 1	School Characteristics	Student Background
		Cohort 1			
Direct	Three-year total school experiences	---	.191	.575	.443
Direct	Total achievement, spring year 3	1.309	.762	.000	.446
Indirect	Total achievement, spring year 3	---	.250	.753	.579
Total	Total achievement, spring year 3	1.309	1.011	.753	1.025
		Cohort 2			
Direct	Three-year total school experiences	---	.145	.755	.388
Direct	Total achievement, spring year 3	.223	.829	1.043	.703
Indirect	Total achievement, spring year 3	---	.032	.169	.087
Total	Total achievement, spring year 3	.223	.861	1.211	.789
		Cohort 3			
Direct	Three-year total school experiences	---	.152	.690	.420
Direct	Total achievement, spring year 3	.691	.879	.000	.633
Indirect	Total achievement, spring year 3	---	.105	.476	.290
Total	Total achievement, spring year 3	.691	.984	.476	.923
		Cohort 4			
Direct	Three-year total school experiences	---	.136	.381	.413
Direct	Total achievement, spring year 3	.271	.856	.000	.844
Indirect	Total achievement, spring year 3	---	.037	.103	.112
Total	Total achievement, spring year 3	.271	.893	.103	.956

Note. — The estimate of an effect is zero (indicated by .000) when the estimate under the saturated model was not significantly different from zero. In that case, the unsaturated model was adopted, with the corresponding parameter assumed to be zero.

The dashes (---) indicate that the parameter is not defined under the saturated model.

Table C-4

Estimates of Metric Effects for the Models of Figure 3-3

Endogenous Variable	Predetermined Variable				
	Three-Year Total School Experiences	Reading Achievement Fall Year 1	Math Achievement Fall Year 1	School Characteristics	Student Background
Cohort 1					
Three-year total school experiences	---	.166	.072	.547	.428
Reading achievement, spring year 3	1.383	.303	.439	.000	.586
Math achievement, spring year 3	.850	.311	.588	.000	.000
Cohort 2					
Three-year total school experiences	---	.097	.067	.766	.378
Reading achievement, spring year 3	.000	.703	.272	.929	.705
Math achievement, spring year 3	.316	.332	.587	1.195	.371
Cohort 3					
Three-year total school experiences	---	.112	.059	.708	.406
Reading achievement, spring year 3	.452	.838	.089	.607	.521
Math achievement, spring year 3	1.024	.328	.616	.000	.000
Cohort 4					
Three-year total school experiences	---	.098	.051	.380	.394
Reading achievement, spring year 3	.000	.756	.188	.000	.962
Math achievement, spring year 3	.000	.287	.498	.000	.659
Math achievement, spring year 3*	.320	.255	.482	.000	.525

*Estimates allowing the effect of X on M to be nonzero. The estimated effect of X on M is barely nonsignificant ($p = .056$).

Table C-5

Elements of the Covariance Matrix of the Six-Grade Synthetic Cohort for Which Data Are Available

Background	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Student background	1-6	1	1	1-2	1-2	1-3	1-3	1-4	2-4	2-5	3-5	3-6	4-6	4-6
2. Total achievement, fall year 1		1	1	1	1	1	1	1						
3. School experiences, year 1			1	1	1	1	1	1						
4. Total achievement, spring year 1				1-2	1-2	1-2	1-2	1-2	2	2				
5. School experiences, year 2					1-2	1-2	1-2	1-2	2	2				
6. Total achievement, spring year 2						1-3	1-3	1-3	2-3	2-3	3			
7. School experiences, year 3							1-3	1-3	2-3	2-3	3			
8. Total achievement, spring year 3								1-4	2-4	2-4	3-4	3-4	4	4
9. School experiences, year 4									2-4	2-4	3-4	3-4	4	4
10. Total achievement, spring year 4										2-5	3-5	3-5	4-5	4-5
11. School experiences, year 5											3-5	3-5	4-5	4-5
12. Total achievement, spring year 5												3-6	4-6	4-6
13. School experiences, year 6													4-6	4-6
14. Total achievement, spring year 6														4-6

Note. — The range of numbers in a cell indicates the interval of cohorts that provide data on the relevant covariance or variance. Empty cells (above the principal diagonal) have no sample data. Entries below the diagonal are merely omitted.

REFERENCES

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